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A Statistical Approach
to Textual Criticism,
with special reference to the
Peshitta of the Old Testament

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Volume 1

ABSTRACT

The general properties of manuscript traditions are considered first. A mathematical model is proposed for the history of a textual tradition as a stochastic "birth-and-death" process. Thus we may check presuppositions which orthodox (i.e. Maasian) textual critics habitually make without self-sufficient grounds, e.g. on the likelihood that a ms of given date will prove to derive from an extant ancestor and have no independent value.

The thesis is mainly devoted to the analysis of particular traditions. The usual representation of the ms inter-relations is a stemma, or family-tree. I found that that method, although logically sound, rested on assumptions which were usually unrealistic. A new representation is proposed, viz a two-dimensional map, on which each ms is represented by a point, and the distances between points correspond to the degrees of textual divergence between mss. The map helps in recovering the textual history and choosing between rival readings. Four examples follow, on texts from Cyprian; Aeschylus, the Vulgate and St. Luke's Gospel.

This new method is then applied to the textual criticism of the Peshitta Psalter, the collations of W. E. Barnes being employed. Critical rules are obtained, and defended on philological grounds. The main conclusions are: (a) this Psalter is not the resultant of several translation attempts, but a single version; (b) the original reading sometimes survives in the Florence codex (9a1) alone; (c) Syriac-speaking Fathers had virtually the same Peshitta text as our mss, but occasionally used other sources; (d) the translation seems to be a Christian rather than a Jewish work. The history of the text is outlined and related to the critical rules. The application of these is illustrated in annotations to the text. Sundry emendations and comments on unexpected renderings are also offered.

There follow a review of other numerical studies in textual criticism, and an Appendix on the Peshitta of Jeremiah xlvi-li.

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* i.e. A:1. Similar abbreviations are used throughout this Plan of Contents.

P R E F A C E

This study springs from my having occupied myself, for as long as I care to remember, both with mathematics and with ancient (particularly biblical) literature. After some deliberation, I decided that the field within which I could most usefully combine these two interests was textual criticism - which, although it may often be thought of as one of the most arid aspects of biblical and classical scholarship, is still basic to them all.

The manner in which the research progressed has caused the thesis to fall into three sections. While casting about in search of a theme, I did some work (with no mathematical content) on the Syriac version of Jeremiah; that work now appears in Section C. When textual criticism emerged as my main topic, I first accepted without question the 'orthodox' view, that manuscript traditions could best be represented in the form of a family-tree; I therefore undertook, within that framework, a mathematical investigation of the transmission of texts, and Section A is the result. Later, however, I learnt that in many textual traditions, and particularly in those of the biblical texts which were my primary concern, the network of relationships between the mss was too complicated to be expressed by a family-tree adequately. Section B, which is divided into eleven chapters, is an account of my effort to devise an alternative system, which was eventually applied to the Syriac Psalter.

It will now be plain - but the point should be driven home - that the thesis has nothing to do with the statistical study of authorship (as applied to problems of disputed authenticity, source analysis, and so on), a topic which has received in recent years a great deal of publicity. If the reader has reservations about the validity of many of these studies (and I should not blame him if he did), I trust that he will not on that account be suspicious of what is offered here.

The fact that the work has to be judged on both the mathematical and the linguistic level, has caused the task of formulation to bristle with problems. In general, I have done my best to write in such a way that the thesis should be intelligible both to the mathematician and to the literary scholar. There are, however, many points at which one has no choice but to enter into technicalities in one or the other sphere; if the reader is temporarily bemused, he will be able to judge for himself where to break off for a while, picking up the thread again later. As a very rough generalisation, he will find himself, as he proceeds through the thesis, progressively more at home if he is primarily a philologist, and progressively less if his main interests are mathematical.

In seeing this project through, I have had cause to be indebted to more people than I can name, and it is a truly invidious task to decide whom to single out for special

mention. I have benefited greatly from discussions with Prof. D.V. Lindley, Father M. Bévenot, S.J., Dr. S.P. Brock, Dr. R.D. Dawe and Mr. J.G. Griffith. Among those teachers without whom I would never have been in a position to begin the thesis, I recall with gratitude Mr. E.J. Frank and Mr. A.E. Goodman; as for Father S. Bullough, O.P., and Prof. D.W. Thomas, it is only to their memory that I can pay tribute. The staff of Dr. Williams's Library have extended to me, with unfailing courtesy, much valuable help. Computer facilities were provided, towards the beginning of the project, by the Polytechnic of North London, but the bulk of the calculations were performed at the University of London Computer Centre. The financial support of the Department of Education and Science, from 1967 to 1970, is gratefully acknowledged; in the subsequent period, the award, made by the University of Cambridge in 1971, of the First Tyrwhitt's Hebrew Scholarship and the Mason Prize for Biblical Hebrew, saved the enterprise from foundering altogether. My friend Mr. Sidney Corob has done me a great service by allowing me to make use of his photocopying facilities in preparing the major part of the thesis for submission. Finally, my hearty thanks are due to my supervisor, Mr. Raphael Loewe, for his invaluable guidance and for his merciless but genial criticism. In the last analysis, however, the responsibility for the whole work is my own.

LONDON
25 September 1973

SECTION A: THE PROPERTIES OF TEXTUAL TRADITIONS*

References

- M.S. Bartlett, "An introduction to stochastic processes", 2nd ed., Cambridge 1966.
- P. Maas, "Textual criticism", translated by B. Flower, Oxford 1958.
- G. Pasquali, "Storia della tradizione e critica del testo", 2nd. ed., Florence 1952.
- L.D. Reynolds and N.G. Wilson, "Scribes and Scholars: A Guide to the Transmission of Greek and Latin Literature", Oxford 1968 [cited as R-W].

§1. Introduction

The prospective editor of an ancient or mediaeval text can hardly hope to be so fortunate as to have access to the original itself. Instead, he will have to base his work on those surviving manuscripts which are ultimately derived from it. Of these there may be many dozens or even hundreds. He will therefore seek to classify these extant witnesses to his text, and to determine the historical relations between them. This information will be valuable at points in the text where the mss are divided between different variant readings; for if he can trace the process whereby the text was transmitted, he will be helped thereby in trying to decide whether a reading attested by a given set of mss goes back to the original, or whether it is an unauthentic

* The gist of a much earlier version of this Section was presented to the Journal Club, at the Department of Statistics, University College London, on 23rd November 1970.

variant which arose somewhere along the line. Our task in this Section will be to study, on a mathematical footing, the process whereby a manuscript tradition grows up, in order to shed some light on various questions which have a serious bearing on textual criticism but cannot be solved by traditional methods, and to check some of the pre-suppositions which textual critics habitually make.

An example of the sort of problem which will concern us here, is furnished by the following celebrated controversy. As we shall presently explain, it has long been a common practice to arrange the mss of a given text in the form of a family-tree (technically termed a stemma). In 1928, the Romance scholar Joseph Bédier launched a violent attack on the accepted procedures whereby stemmata were reconstructed¹. One of his principal arguments was that, out of the 110 stemmata that he had seen presented in critical editions of mediaeval French texts, no less than 105 were two-branched; this heavy preponderance of the two-branched type of stemma, he claimed, was absurd, and the methods which had led scholars to such a result must have been fallacious. The point has provoked a great deal of controversy, which has not died down to this day, and it will occupy us presently; but what we wish to point out now is the methodological problem raised, namely that when the methods of textual criticism are them-

1. Romania (1928), pp. 168 ff.

selves brought into question, we cannot appeal to those same methods in order to decide the matter. More generally it can be stated that the axioms upon which one approach to textual criticism depends will lead to certain self-consistent conclusions, while a different set of axioms would lead to different conclusions, which are equally self-consistent. The scholar might find it difficult to justify his own particular choice of axioms, without indulging in a circular argument. But if we can set up a mathematical model for the evolution of textual traditions, we may find that we can appeal to its impartial authority.

Let us survey the stages by which our investigation will proceed. First, we set up a mathematical model, and try to estimate the statistics that we shall need in order to be able to utilise it. Then we shall consider briefly the literary scholar's view of textual criticism, so that we can understand what questions present themselves, and why. Only then will we be in a position to apply the model in a manner which may prove useful to him; and it will be found that many of his queries raise rather complex mathematical points.

§2. The model

We shall regard the evolution of a ms tradition as a 'birth-and-death' process in continuous time: the 'birth' of a ms will mean its completed production, and 'death' its disappearance.

Let us set up certain formal definitions:

The original is the first ms of the tradition, and the point when it is complete (and therefore liable to be copied) is denoted by time $t=0$.

Birth rate. For all $t > 0$, there exists a function $\lambda(t)$ such that any given ms extant at time t has probability $\lambda(t)\delta t$ of reproducing (i.e. being copied) in the ensuing time interval $[t, t+\delta t]$.

The immediate ancestor, or exemplar, of any ms M (except the original) is defined as that ms which was 'alive' when M was produced, and which shows the least number of textual divergences from M .

Death rate. For all $t > 0$, there exists a function $\mu(t)$ such that any given ms extant at time t has probability $\mu(t)\delta t$ of disappearing from circulation¹ in the time interval $[t, t+\delta t]$.

End of process. We may take any time τ that we please ($\tau > 0$) to be the end of the process; by so doing, we shall obtain the situation existing at that time. The birth component of most manuscript traditions virtually ceased soon after the invention of printing, but the death component continues to the present day², which is the time that we shall usually denote by τ .

In this connection we note that it is possible for us to restrict our consideration to mss dating from before an arbitrary date t_0 ($0 < t_0 < \tau$), by regarding them as the product of a process with birth-rate $\lambda_0(t)$ and death-rate

1. It seems best to regard papyri as 'dead', for they were lost so many centuries ago, and not recovered until the evolutionary process was as good as over.
2. Manuscripts were lost, for example, through a fire in Turin in 1904; for other examples, see Pasquali, p.44.

$\mu_0(t)$, where $\lambda_0(t) = \lambda(t)$ for $0 \leq t < t_0$, $\lambda_0(t) = 0$ for $t \geq t_0$, $\mu_0(t) = \mu(t)$ for all t .

Independence assumptions. It is further assumed that the transition probabilities $\lambda(t)\delta t$ and $\mu(t)\delta t$, to which each ms is subject, are independent of each other and of the behaviour of every other ms.

We may mention briefly some shortcomings and other points relating to this model. Our definition of 'original' may cause difficulty when an author publishes the same work in two or more editions, each of which can claim to be 'original'. The birth-rate λ has been made a function of time alone, though there must have been other factors (e.g. locality). In particular, age has been neglected: the systematic effect of age on fertility is of course far less serious in relation to mss than in biological contexts, but variation in the style of writing over the centuries must have caused many a scribe to prefer to copy a recent rather than an ancient ms¹. Our definition of affiliation is admittedly cumbersome; but it defines uniquely the exemplar of each ms, and thus renders the model still valid for traditions wherein copyists have sometimes availed themselves of two or more sources. Our definition of μ implies that the 'death' of a ms is instantaneous; however, a ms may

1. Of particular importance is the change which took place in cent. ix, from uncial to miniscule script, in the writing of Greek books. From that time onward, a scribe would seek an exemplar written in the miniscule script, with which he was more familiar. (Perhaps we could approximate this effect by assuming a peak in the death-rate - a ms in an obsolescent script being considered virtually 'dead' - followed by a compensatory peak in the birth-rate.)

become mutilated, in part only; we may perhaps define a ms to be 'alive' if the greater part of the text is intact.

Furthermore, we have neglected the effect of age on μ ; but, as in the case of λ , although age is relevant (in that a very old ms is liable to fall apart), it is far less so than in biological applications. Finally, our assumption that all the 'births' and 'deaths' are independent, is in many respects unrealistic; in particular, there was during the Renaissance a tendency for scholars to throw away old mss once they had been copied.

In sum, one cannot deny that this model suffers from many deficiencies; nevertheless, it can be defended as a first approximation.

§3. Simulated traditions

It seemed expedient, before proceeding to the study of real textual traditions, to generate by computer a series of artificial ones, of which all details of the "history" would be known.

The model which was adopted for these experiments (and which can be regarded as a particular case of the type described in §2) consists of a process with two phases. In the former, in which t ranges from 0 to 1000 (sc. years), there is a constant birth-rate λ and a constant death-rate μ . There follows a destruction phase, in which every ms extant at time $t=1000$ has probability ν of

perishing¹. This model is to represent a "typical" tradition which begins ca. 500 A.D.² and continues until the text is first printed (ca. 1500 A.D.), after which no more manuscript copies are produced, and every ms then has probability $(1-\nu)$ of surviving to the present day and coming to the attention of scholars. To these three parameters the following values were assigned: $\lambda = 0.0135$, $\mu = 0.0115$, $\nu = 0.3$.

This choice of values was prompted by various considerations:

(i) The value 30% for the probability of a ms being lost between ca. 1500 and the present day appeared not unreasonable.

It would of course be desirable to base our estimate of ν on more objective grounds. One possibility is to assess, for a given locality (or set of localities), how many books were in existence there in 1500, and how many of these survive today. For estimating the latter, we have an excellent source in a work of N.R.Ker³, which sets out to identify all surviving mediaeval mss

1. The duration of this phase is immaterial from the mathematical point of view; it may however be conceived of as being about 500 years (cf our "typical" tradition).
2. This date was chosen because it lies within the range of the starting-points of the various traditions, classical and mediaeval, to which it is hoped that our work will be applied.
3. "Medieval Libraries of Great Britain", 2nd ed., London 1964.

that once belonged to some particular college or religious house etc. in Great Britain. The former quantity, however, is elusive indeed. A promising source is the *Collectanea* of John Leland¹, who travelled about England and Wales from ca. 1535 to 1542 and made notes of books which he found in various libraries; but as Leland's lists are far from exhaustive, and often fail to give sufficient detail to enable a ms to be confidently re-identified today, the sort of estimate that we require can hardly be obtained by a straightforward comparison of Leland with Ker. An alternative is to consult various mediaeval catalogues that survive and have been published²; some of these were compiled at a time close to 1500 (we have, for example, a catalogue dated 1506 for Exeter Cathedral). It would be desirable to pool together the evidence of a fair number (at least ten, say) of these catalogues, in order that one may reduce the risk of being unduly influenced by factors pertaining to any one particular library and thence obtaining a freak result. At all events, the resourceful specialist may well be able to suggest³ a realistic value for ν ; but I have not been able to take

1. "Joannis Lelandi antiquarii de rebus Britannicis *Collectanea*", ed. T. Hearne (Oxford 1715; 2nd ed., London 1774).

2. References will be found in Ker, *op. cit.*

3. One of the difficulties which he will encounter is that, as we are warned on p. xxviii, Ker's compilation does not record many hundreds of English monastic mss which are extant but for which there is no satisfactory evidence for assigning to any particular mediaeval library; to take account of this point, our eventual estimate of ν would have to be adjusted downwards.

the matter further myself. My impression now is that 0.3 is somewhat - perhaps seriously - on the low side; but, having regard to the particular purposes to which these simulated traditions will be put, I have not thought it necessary to re-work all the experiments, with a higher value for ν .

(ii) The mean date of the surviving ms population was to be in the region of 1400 A.D. This seemed a 'typical' value: among the traditions referred to in Table A.5, (see p. A:27), the mean date of the extant mss is earlier than 1400 in some and later in others.

We may estimate the average date of origin of the members of a population subject to constant birth- and death-rates λ and μ , by resorting to a deterministic approximation. At time t , the population size will be $\varepsilon = e^{(\lambda - \mu)t}$. Let the average date of origin within that population be u . In the time interval $[t, t + \delta t]$, the population will be reduced through death by $\mu \varepsilon \delta t$, and will receive an increment of $\lambda \varepsilon \delta t$; the date of origin of that increment will be t . If $u + \delta u$ denote the average date of origin for the population as it is at time $t + \delta t$, then

$$[1 + (\lambda - \mu)\delta t] \varepsilon (u + \delta u) = [1 - \mu\delta t] \varepsilon u + [\lambda\delta t] \varepsilon t,$$

whence the differential equation $\frac{du}{dt} = (t - u)$,

of which the solution, subject to the condition that $u=0$ when $t=0$, is

$$u = t - \frac{1}{\lambda} (1 - e^{-\lambda t})$$

Thus after the birth-and-death process has run its course for 1000

years, with $\lambda = 0.0135$, the average date of the ms population may be estimated at a point 926 years later than the beginning of the process. As we take our starting-point about 500 A.D., we reach an average date ca. 1425 for the set of mss which result from the 'birth-and-death' phase, and the same estimated average date can be put forward for the mss that are left after the 'destruction' phase too is complete.

(iii) It was further desired that among the traditions that survived, a small but solid proportion (say 5%) should consist of one ms alone, and a similar proportion should contain a hundred or more mss.

This desideratum was suggested by the surprising amount of variation in the size of different manuscript traditions of the best-known classical authors. Cases in which a text has been preserved in a single ms are not infrequent, obvious examples being Cicero: De Republica and Livy 41-45, while, at the other extreme, populations of more than 100 mss are commonly met with (see Table A.5); divergence of this order may even occur among different works of one and the same author (e.g. Euripides). The possibility seemed worth investigating that a factor underlying this diversity was the well-known variability¹ between different realisations of a birth-and-death process with given birth- and death-rates, or in other words that the phenomenon was due largely to chance rather than deliberate choice².

1. See Bartlett, p. 75.

2. R-W discuss the theory (pp. 45f) that a limited school syllabus was at some time drawn up by some prominent schoolmaster, and came to be adopted by all schools, while works that had not been included therein became neglected; but as R-W observe, there is little solid evidence in its favour.

For a stochastic birth-and-death process, with constant λ, μ , the probability that the population at time t will be n , is given by the coefficient of z^n in the p.g.f.

$$\left[\frac{\mu \varepsilon (z-1)}{\lambda z - \mu} - 1 \right] / \left[\frac{\lambda \varepsilon (z-1)}{\lambda z - \mu} - 1 \right] \quad (\varepsilon \text{ again means } e^{(\lambda - \mu)t})$$

according to a well-known result¹. If we add a destruction phase, in which each individual has chance ν of perishing, then we shall obtain a p.g.f. for the population at the end of this second phase by substituting $\nu + (1-\nu)z$ for \bar{z} . This p.g.f. is $(Az+B)/(Cz+D)$, where

$$A = (\mu \varepsilon - \lambda) (1-\nu)$$

$$B = (\mu \varepsilon - \lambda)\nu + \mu(1-\varepsilon)$$

$$C = \lambda(\varepsilon-1)(1-\nu)$$

$$D = \lambda(\varepsilon-1)\nu + (\mu - \lambda\varepsilon)$$

If we now choose to confine ourselves to cases in which the population does not become extinct, we obtain a new p.g.f. for the population size at the end of the two-phase process when it is known to exceed zero, namely $-(\lambda-\mu)z/(Cz+D)$; and the probability that the population size will exceed n , reduces to $(-C/D)^n$. For the above values of λ, μ, ν , we find $C=+0.06037$, $D=-0.06237$; the probability that the population will be exactly 1, is 3.2%, and the probability that it will be 100 or more, is 3.9%.

It is noteworthy that with these values of λ, μ, ν , the probability that the tradition ultimately escapes extinction is

1. Bartlett, p. 74.

no more than 16%. This pessimistic picture is in keeping with what we know of classical traditions. What has come down to us is only a fraction of what existed originally; to take just one example, the Eclogae of Stobaeus (dating probably from the latter half of cent. v) and other sources give us the names and sundry fragments of about 100 lost dramas of Sophocles, and only seven have survived in their entirety.

Thus it can be claimed that the values adopted for λ , μ , ν are acceptable; but I would acknowledge the need for further experiments, in which different values could be tried out.

The artificial traditions were produced on a computer, by the method which Bartlett describes (p. 75). The computer was programmed to continue generating new series of pseudo-random numbers, and thence new populations, until fifteen traditions had emerged which were not extinct at the end of the two-phase process. No less than forty-six attempts were required to yield these fifteen extant traditions. Table A.1. gives the final population, and also the total number of mss which ever existed within the tradition, whether alive or dead by the end of the process.

It is not of course suggested that the two-phase model from which these artificial traditions were generated, would approximate tolerably the conditions under which any real text was transmitted. Nevertheless, we shall find the fifteen experiments useful in three ways. Firstly,

TABLE A.1.

<u>Experiment No.</u>	<u>Final Population</u>	<u>No. of mss ever existing</u>
1	1	44
2	31	205
3	15	103
4	12	153
5	16	159
6	35	286
7	2	63
8	29	263
9	57	373
10	16	109
11	45	212
12	15	77
13	21	173
14	92	630
15	10	72

we sometimes have to adopt an approximation of some sort in our analysis - e.g. to use a deterministic model instead of the stochastic one - and we can check our results by reference to the experiments. Secondly, if there are any properties which most ms traditions share (e.g., as some claim, a tendency to bipartite structure), we shall expect our artificial traditions to share them too; thus there is a possibility of drawing negative conclusions about allegedly general phenomena. Finally, they give us a complete record, which can be "frozen" at any point in the past, and so allow us to visualise situations as they develop. We may thus supplement whatever results we obtain by analytic means.

§4. An attempt to estimate parameters by the method of
maximum likelihood

Let us consider a model process in two phases, not unlike the one adopted for our computer experiments.

The former phase will be a 'birth-and-death' process with constant λ, μ , lasting from the creation of the original until 1500 A.D.; the latter will be a destruction process, in which a given ms that is extant in 1500 A.D. will have probability ν of failing to survive and to appear in a catalogue. In contrast to our work in §3, the starting-point will not necessarily be taken to be 500 A.D., and the three parameters λ, μ, ν will be treated at the outset as unknown, our task being to estimate them for a particular real tradition.

One's approach to the problem is of course conditioned by the nature of the data available. We cannot, for example, follow the method described in Bartlett, pp. 279 ff, which requires a knowledge of every birth and death and of the time at which it occurred. In our present problem, all we can hope for is a list of the mss which now survive, and an approximate dating for each. In general, a ms can be assigned to a particular century - though we sometimes cannot be confident even of that, and have to quote a wider margin; only rarely can it be dated more exactly¹. Thus if we take

1. For example, C. Holzinger, "Sur la date de quelques manuscrits d'Aristophane", in *Mélanges offerts à M. Emile Chatelain* (Paris 1910), pp. 204-218, shows how the watermark of a paper ms may enable us to date it within a shorter time interval, such as a third of a century. Again, a scribe may record on the ms itself the year in which he wrote it; but this practice was all too rare.

the time at which the original was composed (this date is assumed to be known, and in most cases we do in fact know it pretty accurately) as $t=0$, our data may be formulated as a list of the extant mss, classified according as they originated in time intervals $[0, t_1]$, $[t_1, t_2]$, and so on, where t_1, t_2, \dots, t_n are n suitable fixed points, which will normally be 100 years apart. t_n will normally represent the date 1500 A.D. and will be deemed to mark the end of the 'birth-and-death' phase of the process; any ms 'born' after t_n will have to be ignored.

Before he proceeds any further, the reader is warned that the work of this section turned out, at least as far as its original purpose is concerned, to be of little practical utility, and has not been appealed to elsewhere in the thesis. I have nevertheless decided to give an account of it, because it is of mathematical interest, and may be capable of being applied with greater success in other fields.

Suppose that n suitable points in time (t_1, t_2, \dots, t_n) are specified. If we then envisage a two-phase process, as set forth above, then we may define a set of n random variates M_1, M_2, \dots , where M_j denotes the number of mss that are extant at the end of the process and were produced within the time interval $[t_{j-1}, t_j]$ ¹. We may further express, in terms of λ, μ, ν , the probability that M_1, M_2, \dots

1. In defining M_1 , we may put $t_0=0$.

will assume a given set of values m_1, m_2, \dots . That probability, as we shall now prove, is given by the coefficient of $x_1^{m_1} x_2^{m_2} \dots x_n^{m_n}$ in the expansion of

$$G(x_1, x_2, \dots, x_n) = \frac{U + \sum_{j=1}^n A_j x_j}{V + \sum_{j=1}^n B_j x_j},$$

where $A_1 = \mu e^{\lambda t_1} - \lambda e^{\mu t_1}$, and $A_j = \mu e^{\lambda t_j} - \lambda e^{\mu t_j} - \mu e^{\lambda t_{j-1}} + \lambda e^{\mu t_{j-1}}$ for $1 < j \leq n$;

$B_1 = \lambda(e^{\lambda t_1} - e^{\mu t_1})$, and $B_j = \lambda(e^{\lambda t_j} - e^{\mu t_j} - e^{\lambda t_{j-1}} + e^{\mu t_{j-1}})$ for $1 < j \leq n$;

$$U = \frac{\mu - \lambda \nu}{1 - \nu} e^{\mu t_n} - \mu e^{\lambda t_n}$$

$$V = \frac{\mu - \lambda \nu}{1 - \nu} e^{\mu t_n} - \lambda e^{\lambda t_n} \dots \dots \dots \text{I}$$

In order to prove this result, we first prove the following lemma.

If we suppose a birth-and-death process in continuous time, with one original member, λ and μ being constant, then the probability that at time t the original member still survives and has j extant descendants is given by the coefficient of xy^j , and the probability that the original member has perished and that j descendants thereof survive is given by the coefficient of y^j , in the expansion of

$$Q(x, y, t) = \frac{(\mu - \lambda)x + (\mu e^{\lambda t} - \lambda e^{\mu t} + \lambda - \mu)y + \mu(e^{\mu t} - e^{\lambda t})}{\lambda(e^{\lambda t} - e^{\mu t})y + (\mu e^{\mu t} - \lambda e^{\lambda t})} \dots \dots \dots \text{II}$$

Proof of lemma. Let $R(z, t) = \sum_{j=0}^{\infty} r_j(t) z^j$ be a generating function in which $r_j(t)$ is the probability that at time t , the original member will be extant together with j descendants.

The following transition probabilities will operate in the interval $[t, t+\delta t]$:

$\mu\delta t$:	death of original
$\lambda(1+j)\delta t$:	creation of one new descendant
$\mu j\delta t$:	death of one descendant

Thus in that interval,

$$z^j \rightarrow z^j(1-\mu\delta t) + \lambda(1+j)(\delta t)(z^{j+1}-z^j) + (\mu j\delta t)(z^{j-1}-z^j)$$

It follows that R satisfies the Lagrange linear equation

$$\frac{\partial R}{\partial t} + (1-z)(\lambda z - \mu) \frac{\partial R}{\partial z} = (\lambda z - \lambda - \mu)R,$$

of which the subsidiary equations are

$$\frac{dt}{1} = \frac{dz}{(1-z)(\lambda z - \mu)} = \frac{dR}{R(\lambda z - \lambda - \mu)}$$

Equating the first and second terms, we obtain one integral:

$$u = \log(1-z) - \log(\lambda z - \mu) + (\lambda - \mu)t \dots \dots \dots \text{III}$$

and equating the second and third, we obtain another:

$$v = (\lambda - \mu) \log R - \mu \log(1-z) + \lambda \log(\lambda z - \mu) \dots \dots \text{IV}$$

The general solution is $\Phi(u, v) = 0$, where Φ is an arbitrary function.

We may determine Φ through the fact that $R=1$ when $t=0$. For if we put $t=0$,

$$u = \log(1-z) - \log(\lambda z - \mu),$$

$$v = \lambda \log(\lambda z - \mu) - \mu \log(1-z),$$

Whence $\frac{\lambda u + v}{\lambda - \mu} = \log(1 - z)$, $\frac{\mu u + v}{\lambda - \mu} = \log(\lambda z - \mu)$,

so that the particular solution that we require is

$$\Phi(u, v) = \lambda \exp\left[\frac{\lambda u + v}{\lambda - \mu}\right] + \exp\left[\frac{\mu u + v}{\lambda - \mu}\right] - \lambda + \mu = 0 \dots\dots\dots V$$

Substituting in (V) our expressions for u and v in (III) and (IV), we find after a little manipulation:

$$R(z, t) = \frac{\lambda - \mu}{(\lambda z - \mu)e^{\mu t} + (1 - z)\lambda e^{\lambda t}}$$

Let us now turn from R to another function

$S(z, t) = \sum_{j=0}^{\infty} s_j(t) z^j$, where $s_j(t)$ is the probability that the total population (consisting of the original, if still 'alive', and all extant descendants) will be j . The function $S(z, t)$ is well-known¹, being:

$$S(z, t) = \frac{\mu(1 - z)e^{\lambda t} + (\lambda z - \mu)e^{\mu t}}{\lambda(1 - z)e^{\lambda t} + (\lambda z - \mu)e^{\mu t}}$$

Now the function $Q(x, y, t)$ can be expressed in terms of R and S ;

$$\begin{aligned} \text{for } Q(x, y, t) &= x \sum r_j y^j + \sum (s_j - r_{j-1}) y^j \\ &= (x - y) R(y, t) + S(y, t), \end{aligned}$$

and the lemma follows.

We now proceed to the proof of (I).

1. Bartlett, p. 74.

Let us first consider the population as it is at time t_1 . The probability that the number of mss then in existence will be n , is given by the coefficient of z^n in the function $S(z, t_1)$, according to the notation just employed. We may define $\Gamma_1(z) = S(z, t_1)$; moreover we shall find it convenient to use the symbol x_1 instead of z . Thus the p.g.f. for the size of the population at time t_1 is

$$\Gamma_1(x_1) = \frac{A_1 x_1 + U_1}{B_1 x_1 + V_1},$$

where A_1 and B_1 have the meanings assigned in (I), while

$$U_1 = \mu(e^{\mu t_1} - e^{\lambda t_1}), \quad V_1 = \mu e^{\mu t_1} - \lambda e^{\lambda t_1}$$

We now move on to time t_2 , and try to formulate a function $\Gamma_2(x_1, x_2)$ such that the coefficient of $x_1^{m_1} x_2^{m_2}$ will give the probability that at time t_2 there exist m_1 mss which originated before time t_1 , and m_2 later ones.

$$\text{Evidently, } \Gamma_2(x_1, x_2) = \Gamma_1 [Q(x_1, x_2, t_2 - t_1)],$$

Q being the function defined in our lemma. After some (rather tedious) algebraic manipulation, we find

$$\Gamma_2(x_1, x_2) = \frac{A_1 x_1 + A_2 x_2 + U_2}{B_1 x_1 + B_2 x_2 + V_2},$$

where A_2 and B_2 have the meanings assigned in (I),

while

$$U_1 = \mu(e^{\mu t_1} - e^{\lambda t_1}), \quad V_1 = \mu e^{\mu t_1} - \lambda e^{\lambda t_1}.$$

Continuing this line of argument, we may define iteratively $\Gamma_{j+1}(x_1, x_2, \dots, x_j, x_{j+1})$, by substituting $Q(x_i, x_{j+1}, t_{j+1} - t_j)$ for every x_i ($1 \leq i \leq j$) that appears in our expression for $\Gamma_j(x_1, x_2, \dots, x_j)$. Hence we arrive at a p.g.f. Γ_n , for the composition of the population at time t_n , i.e. the end of the birth-and-death phase:

$$\Gamma_n(x_1, x_2, \dots, x_n) = \frac{U_n + \sum_{j=1}^n A_j x_j}{V_n + \sum_{j=1}^n B_j x_j}$$

$$\text{where } U_n = \mu(e^{\mu t_n} - e^{\lambda t_n}), \quad V_n = \mu e^{\mu t_n} - \lambda e^{\lambda t_n}.$$

Finally we consider the population after the destruction phase. We shall obtain the appropriate p.g.f. if, in Γ_n , we replace each x_j by $\nu + (1-\nu)x_j$. Making these substitutions, and dividing both numerator and denominator by $(1-\nu)$, we reach the required result (I).

We derive from this generating function the probability that at the end of the whole process there will survive m_1 mss dating from $[0, t_1]$, m_2 from $[t_1, t_2]$, and so on, and find it to be

$$\frac{M!}{\prod (m_j!)} \prod (B_j^{m_j}) \left\{ \frac{-1}{V} \right\}^{M+1} \left[-U + \frac{V}{M} \sum \frac{A_j m_j}{B_j} \right], \text{ where } M = \sum m_j.$$

For any given tradition, we may substitute in the above expression the values actually observed of m_1, m_2, \dots , obtaining a function of λ, μ , and ν ; by maximising that function we may estimate the three parameters. In practice it is easier to maximise the following expression L' , which consists of the logarithm of the above, minus a constant which does not depend on λ, μ or ν :

$$L' = \sum m_j \log B_j - (M+1) \log (-V) + \log \left[V \sum \frac{A_j^{m_j}}{B_j} - UM \right]$$

Following the method of J.A. Nelder and R. Mead¹, I sought (with computer assistance) to obtain estimates (to be denoted $\hat{\lambda}, \hat{\mu}, \hat{\nu}$) in this way, and was requited with some rather disturbing results:

TABLE A.2

(i) Aristophanes: Clouds

Source J.W.White, "The Manuscripts of Aristophanes", Classical Philology (1906), pp. 9 ff.

Boundaries of time intervals	actual date	B.C. 423	A.D. 1000	1100	1200	1300	1400	1500
	from origin of work	0	1423	1523	1623	1723	1823	1923
No. of surviving mss dating from given interval		0	1	1	3	27	78	

Estimates: $\hat{\lambda} = 0.013221$ $\hat{\mu} = 4.0 \times 10^{-11}$ $\hat{\nu} = 0.999999999$

/ continued

1. "A simplex method for function minimisation", Computer Journal vii(1965), pp. 308-313.

TABLE A.2 (cont.)(ii) Simulated tradition no. 14

Boundaries of time intervals	0	700	750	800	850	900	950	1000
No. of surviving mss dating from given interval	0	1	0	2	12	31	46	
<u>Estimates:</u> $\hat{\lambda}=0.017509$	$\hat{\mu}=7.5 \times 10^{-11}$		$\hat{\nu}=0.9999977113$					
(but in fact $\lambda=0.0135$	$\mu=0.0115$		$\nu=0.3$)					

These estimates of μ and ν , so obviously wrong, would suggest that the death-rate μ was negligible, and that manuscript loss was virtually confined to the second phase, which an enormous number of mss entered, each with a minute chance of survival. Yet it would be uncharitable to berate the computer on that account; for an unchecked birth process - producing a ms population to be measured in millions - followed by a massive destruction, could indeed (theoretically) have yielded the figures submitted. Thus our method has not proved sensitive enough to distinguish the effects of the death component in the first phase from those of the destruction process. The values of $\hat{\lambda}$ make more sense - though in (ii) it is disappointing to find $\hat{\lambda}$ overestimating λ by about 30%.

All in all, having come so far, we must resign ourselves to the prospect of not finding this method of estimating the parameters to be of much practical value.

On the debit side, it has to be performed by computer, and involves the unrealistic assumption of constant birth- and death-rates from antiquity down to the Renaissance; in return, the estimates which it offers of μ and ν are useless, and that of λ , while being of the right order of magnitude, does not appear to be outstandingly accurate. We shall have to approach the estimation problem from another direction.

§5. Other approaches to the problem of estimation

Let us now turn to another method of estimating the birth-rate. This method will take account of the possibility of the birth-rate varying with time; thus $\lambda = \lambda(t)$.

We first set forth the mathematical background. We seek to estimate the mean birth-rate between two points in time, $t=a$ and $t=b$. It is assumed that we know how many mss survive today which were already in existence at time $t=a$; let this figure be denoted x_a . Similarly we may define x_b (which cannot, of course, be less than x_a). Let us denote by y_a the total number of mss which existed at time $t=a$, and define similarly y_b ; y_a and y_b are of course unknown to us today.

Let us now approximate the development of the tradition from time $t=a$ onward, by a deterministic birth-and-death

process, with the same birth- and death-rate ($\lambda(t), \mu(t)$) as the actual stochastic process. On that basis,

$$\begin{aligned} x_a &= y_a \exp \left[- \int_a^t \mu dt \right] \\ y_b &= y_a \exp \left[\int_a^b (\lambda - \mu) dt \right] \\ x_b &= y_b \exp \left[- \int_b^t \mu dt \right] = y_a \exp \left[\int_a^b \lambda dt - \int_a^t \mu dt \right] \\ \therefore \frac{x_b}{x_a} &= \exp \left[\int_a^b \lambda dt \right] = \exp \left[(b-a) \bar{\lambda} \right], \end{aligned}$$

where $\bar{\lambda}$ is the mean value of λ over the interval $[a, b]$.

Hence we have an estimator, which we may denote $\tilde{\lambda}$:

$$\tilde{\lambda} = \frac{\log x_b - \log x_a}{b - a}$$

$\tilde{\lambda}$, then, is based on only two observations, x_a and x_b (i.e. the respective numbers of extant mss pre-dating the times $t=a$ and $t=b$). Moreover, it is not necessary to know the absolute values of a and b - which would involve knowing the date of the original - but only the length of the interval between them; this is just as well, for in some cases (e.g. the Peshitta) we cannot be sure even of the century in which the original first saw the light. We note further that the estimation of $\tilde{\lambda}$ demands no more elaborate equipment than a table of logarithms.

The estimator $\tilde{\lambda}$ cannot be employed indiscriminately. One of the most obvious restrictions is that a minimum value must be set up for x_a , as cases like $x_a=0, x_b=1$ (implying $\tilde{\lambda}=\infty$!) would suggest; in the upshot, 5 was

chosen as a suitable minimum¹. The reliability of $\tilde{\lambda}$ can be further checked through our experimental traditions. For seven of these, $\tilde{\lambda}$ was calculated for the last century of the 1000-year period²; the true value of λ , we recall, is 0.0135. On the showing of these experiments, $\tilde{\lambda}$ is of the same order of magnitude as λ , but ranges between about 80% and 150% of the true value.

TABLE A.3.

Experiment no.	$x_a (a=900)$	$x_b (b=1000)$	$\tilde{\lambda}$
5	5	16	.0116
6	9	35	.0136
8	6	29	.0158
9	12	57	.0156
11	6	45	.0201
13	7	21	.0110
14	15	92	.0181

In order for a test to be provided of the accuracy of $\tilde{\lambda}$ when λ is not constant, five artificial traditions were generated on the computer with a variable birth-rate $\lambda(t) = mt + c$ (where $m = -9 \times 10^{-6}$, $c = 0.01$), there being no death component. In each experiment, the process was to terminate as soon as t reached 1000 or the population

1. It may further be presumed that if one worked with very short time intervals, the accuracy of one's results would suffer; but this does not seem a serious danger, because we shall have little hope of dealing with time intervals shorter than 100 years.
2. The other eight experiments could not be used because less than five mss earlier than $t=900$ have survived.

reached 500. Here are the values of $\tilde{\lambda}$ obtained from these five traditions:

TABLE A.4

Boundary of time interval	True value of $\tilde{\lambda}$	Value of $\tilde{\lambda}$ derived from experiment no:				
		1	2	3	4	5
100	.00865	-	.0110	-	-	-
200	.00775	-	.0076	-	-	.0063
300	.00685	-	.0072	-	.0122	.0061
400	.00595	.0064	.0076	-	.0076	.0069
500	.00505	.0080	.0049	.0083	.0052	.0056
600	.00415	.0041	-	.0032	.0049	.0042
700	.00325	.0043	-	.0031	.0033	.0033
800	.00235	.0019	-	.0018	.0026	.0020
900	.00145	.0016	-	.0011	.0023	-
1000						

Note $\tilde{\lambda}$ was not to be calculated for any time when the population was less than 5 or more than 500; hence the blank entries before or after each column of figures.

Here $\tilde{\lambda}$ ranges between about 75% and 180% of the true value.

These tests encourage us to believe $\tilde{\lambda}$ to be a serviceable estimator for $\bar{\lambda}$, provided that we keep in mind the limits of its accuracy. The results obtained when we apply it to real textual traditions are shown in Table A.5. In that table, the birth-rate estimates for some centuries are doubtless rather inaccurate, but those fluctuations which are particularly marked (e.g. the 'peaks' in (iv) during the ninth,

TABLE A.5.

(i) Aeschylus: Persae

Source: H.W. Smyth, "Catalogue of the Manuscripts of Aeschylus", Harvard Studies in Classical Philology (1933), pp. 1 ff.

t	1300	1400	1500	1600
n	5	32.5	58	63
λ		.01872	.00579	.00083

(ii) Sophocles: Ajax

Source: A. Turyn, "The Manuscripts of Sophocles", Traditio (1944), pp. 1-41.

t	1300	1400	1500	1600
n	6.5	58	128.5	154
λ		.02189	.01796	.00181

(iii) Aristophanes: Clouds

Source: See Table A.2.

t	1300	1400	1500	1600
n	5	27	78	111
λ		.01687	.01061	.00353

(iv) Cyprian: De Unitate

Source: M. Événot, "The Tradition of Manuscripts," Oxford 1961.

t	n	λ
800	1	
900	15	.02708
1000	16	.00064
1100	25	.00046
1200	57	.00824
1300	68	.00176
1400	79	.00150
1500	152	.00654
1600	156	.00026

(v) Bede: Ecclesiastical History of the English People

Source: M.L.W. Laistner, "A Hand-list of Bede Manuscripts", New York 1943.

t	n	λ
800	5.5	
900	15	.01003
1000	25	.00511
1100	42	.00519
1200	91.5	.00779
1300	109	.00175
1400	132	.00191
1500	152.5	.00144
1600	155	.00016

(vi) The Peshitta Version of Genesis

Source: List of O.T. Peshitta Manuscripts, Leyden 1961, pp. 82f. N.B. Fragments and massoretic mss are neglected.

t	n	λ
800	5	
1200	12	.00219
1600	20	.00128
1700	34	.00531
1800	40	.00157
1900	45	.00118

Explanation.

t is measured in years A.D. n is the number of extant mss which are at least as old as t. Those few mss which cannot be dated within narrower limits than a two-century period, are each counted as half a ms in either century; hence such entries as 32.5.

twelfth and fifteenth centuries, and in (vi) during the seventeenth), surely correspond to real historical changes in the birth-rate.

It can further be stated that the survival of a large number of mss from a certain period does not in itself prove that the birth-rate was then particularly high; what matters is their proportion to the number of earlier extant mss.

It is also possible to form an impression of the nature of the fluctuations of $\lambda(t)$ - though not of their extent - from our knowledge of changing cultural conditions. Thus we may suppose that for classical Latin traditions, a graph of $\lambda(t)$ might have been something like fig. A.1.

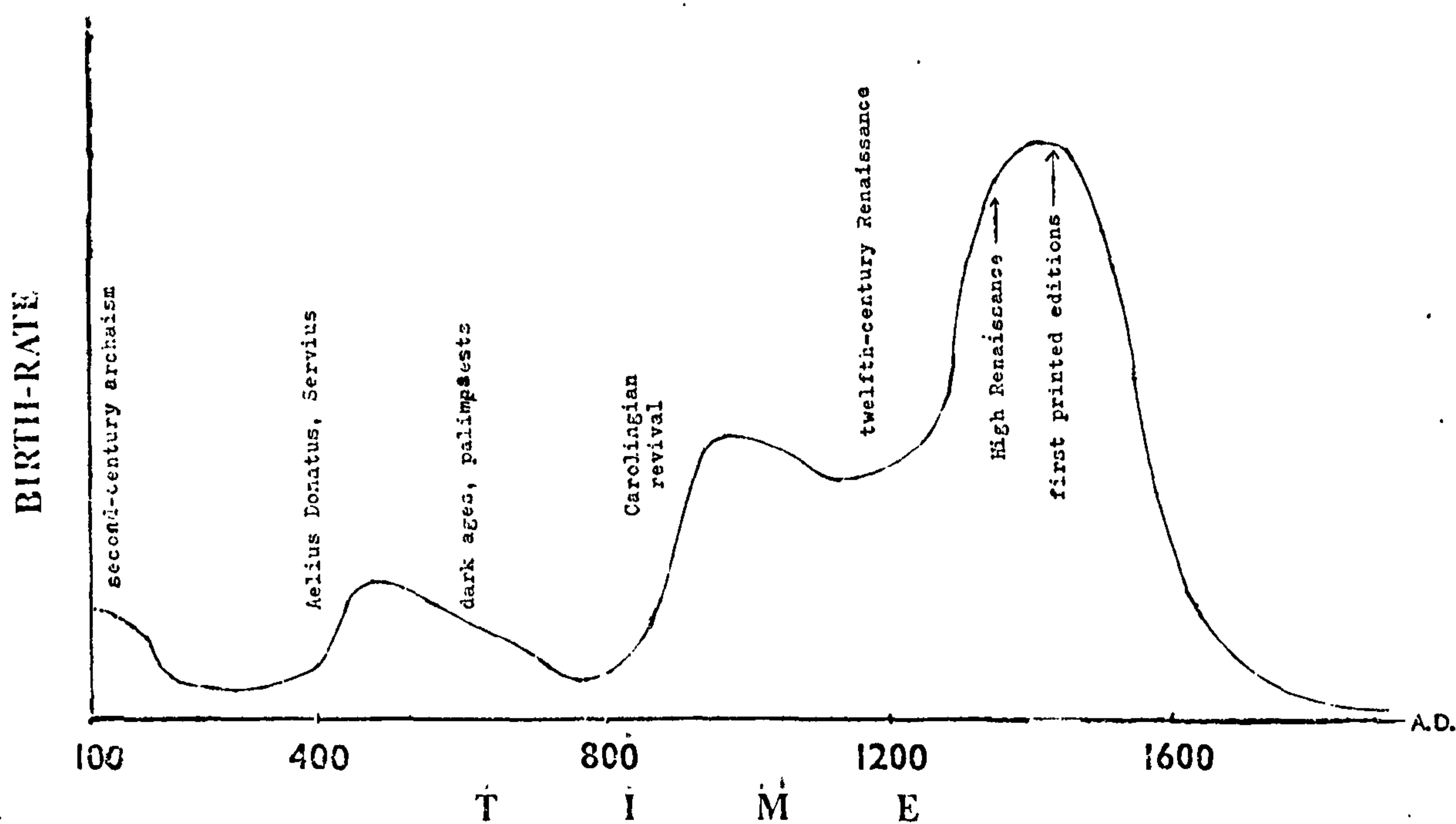


FIG. A.1.

This approach seems to be our only prospect of tracing the movement of λ in the earlier stages of textual transmission.

To sum up, estimation of the birth-rate is to some extent within our reach; and we shall be provided thereby with an index of the interest which was aroused by a given text at different times. As will be shewn presently (§9), the birth-rate also has a bearing on the question of the number of intermediate copies that separate a ms of given date from the original.

For the death-rate, however, I have been unable to devise any acceptable means of estimation. This admittedly creates a serious gap in our treatment, but has not by any means brought it to a halt.

§6. The view-point of orthodox textual criticism

It is now time to consider the manner in which textual traditions are analysed by classical scholars. There is now a great deal of controversy in this area; but the system expounded by Paul Maas can still be said to represent the 'orthodox' view. It sets out to arrange the extant mss in the form of a family-tree, called a stemma, of which the starting-point is the original; the stemma, once established, will provide a sound basis for inference about the original text. The rationale of this method is fully discussed in Chapters 1 and 2 of Section B of the present Thesis.

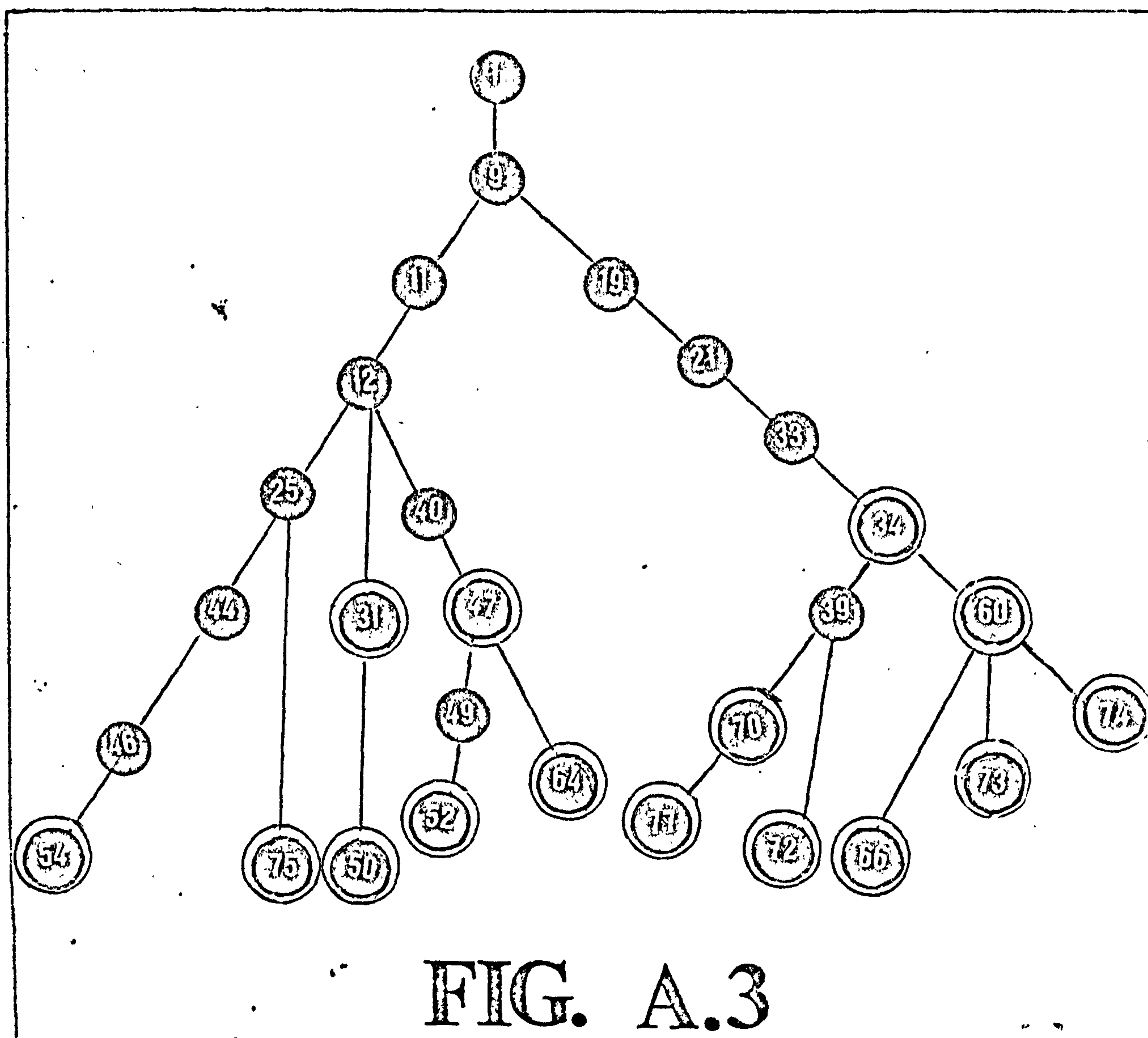
To fix our ideas on the view-point of the textual critic, let us consider the twelfth of our experiments. The complete



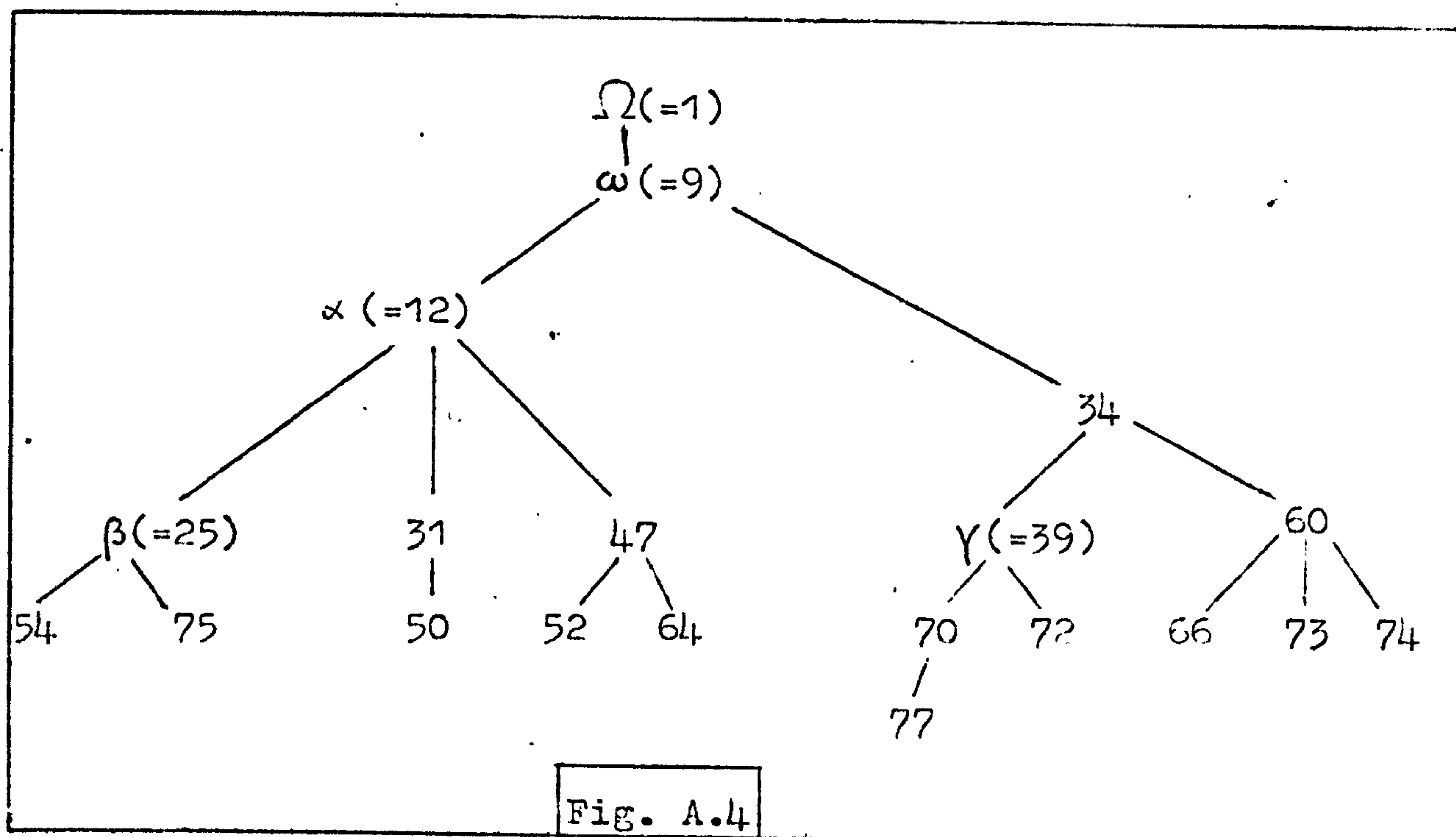
1907

genealogy of all 77 mss involved may be reconstructed from the computer record. It is presented in fig. A.2, where the mss are numbered in the order in which they were 'born', 1 denoting the original. The 'live' mss are distinguished from the 'dead' ones by an outer ring. It is only the former of which the philologist will have direct knowledge - even though they are doubtless far outnumbered by the latter.

Maasian criticism allows the philologist to simplify the tree in various respects. Firstly, he will not be interested in those 'dead' mss that have left no extant descendants; and so the only lines of descent which he will find relevant are as under:



Moreover, he will be largely unaware of how many copies intervene between two mss (e.g. that 54 is a third-generation copy of 25, while 75 is a direct copy). Thus the tree can be further simplified, and only in this final form would it be termed a stemma:



Here Ω represents the original. The latest common ancestor of all the mss (namely 9) is termed the archetype (ω); in our case the archetype is no longer extant. Other lost mss whose former existence can be inferred are represented by branch-points in the stemma, and may be assigned symbols (customarily, Greek miniscules). Even if the scholar succeeds in recovering correctly the interrelations of the extant mss, this is essentially all that he can learn of the full textual history which we saw previously in so much greater detail.

He will now state that all the mss go back to one archetype, which is distinct from the original and from which there is a two-way split in the tradition. On the left-hand side, there is a secondary three-way split (see α); and so on.

It will now be clear that the terms which the scholar applies are essentially retrospective. Ms 9, for example, was not the archetype when it was first produced; it became the archetype by virtue of subsequent events which resulted in the extinction of all mss not descended from it. There is a two-way split from this archetype, but in fact it was copied three times; and α was copied not thrice, but altogether nine times. The situation that is discerned today is very different from that which existed at various points in the past.

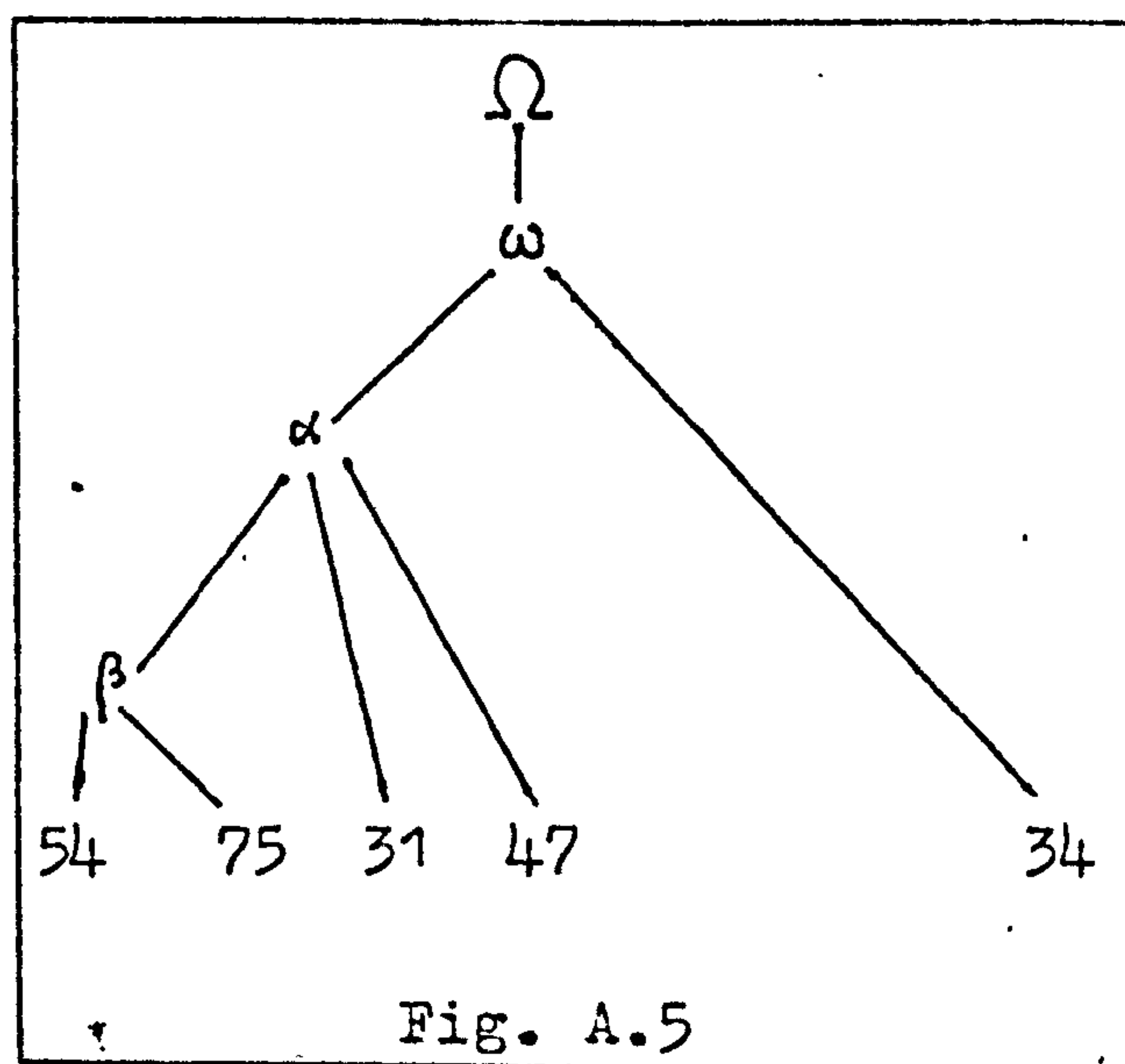
§7. An outline of stemmatic procedure

The first task is to reconstruct the stemma, on the basis of the texts shown by the various mss. One looks for errors which are in themselves unlikely to have been perpetrated by any two scribes independently; if a given set of mss is observed to share such an error, then it may be inferred that they have a common ancestor, in which that error originated. If sufficient errors of this sort (Maas calls them 'conjunctive errors' - p. 43) can be found, the whole stemma can be built up. Suppose, for example, that we were given the fifteen mss shown in fig. A.4, but

knew nothing of their ancestry. After a sufficiently thorough investigation, we would find (according to what we have called the orthodox view-point) errors common to 31,47,50,52,54,64,75, and would deduce that they had a common ancestor (namely α); again, 54 and 75 would share some errors found in no other ms; 52 and 64 would contain all the errors of 47 plus a number of peculiar errors in each, whence we infer that 47 is their latest common ancestor; and so on.

The employment of the stemma for the reconstruction of the original text proceeds by the following stages, which we denote by the Latin terms used by Maas.

Eliminatio codicum descriptorum. We first eliminate from consideration any ms which is descended from, and therefore dependent on, another extant ms; such a dependent ms is termed a 'descriptus'. Thus in fig.A.4 we would discard as descripti nos. 50,52,60,64,66,70,72,73, 74,77, and cut down our stemma accordingly:



Recensio. We then work back in stages from the extant mss, which now form the bottom row of the stemma. At every step we use the rule that the reading of a lost ms is guaranteed by the agreement of any two of its descendants, or by the agreement of any one of them with an outside witness. The text of β for example, could be deduced if 54 agreed with 75, or else if either agreed with 31,34 or 47. In the same way the readings of α , and finally ω , could be reconstructed, wherever the necessary agreements were observed.

Selectio. A special problem arises when (as in our example) the primary split is into just two branches. It will sometimes happen that the mss at the head of either branch (in our case, α and 34) offer two different readings; the stemma will not then help us to prefer either to the other. In such cases we shall have to choose between them on their intrinsic merits. When the stemma has three or more branches, 'selectio' is necessary only in the exceptional case in which no two branches are in agreement.

Emendatio. These procedures cannot take us back farther than ω , which may have been (and, according to the orthodox view, usually was) not identical with Ω , but a descendant thereof, and may therefore have been marred by errors. Thus, having reconstructed the archetype, we shall sometimes need to emend its text by conjecture in order to restore the text of the original.

The logical basis of this method, we repeat, will be dealt with below, and does not concern us here. At all events, a great deal of work in the field of textual criticism does rest on Maasian principles, and so we must go into the questions which they involve.

§8. Eliminatio codicum descriptorum

The student who is confronted with all the mss that make up a given textual tradition, and is wondering which of them can be treated as descripti, would find it useful to have in advance some estimate of the proportion that descripti form of the tradition. However, textual critics themselves are far from being agreed on their policy regarding this question.

On the one hand, Maas writes (p.52): "The oldest existing witness is always completely 'independent', whereas the independence of later witnesses, as against those which are earlier than themselves, must first be proved by 'separative errors'¹. Accordingly in the examination of witnesses with a view to their independence, the right course is to begin with the oldest but one and then to work through to the recentiores² in chronological sequence; these recentiores will for the most part, though of course not always, turn out to be dependent". Thus Maas, when considering a given ms M, would start out by presuming it

1. The term is explained below.

2. i.e. later mss.

to have been derived from one of the older mss now extant, unless a passage could be found where M has the original reading while the older mss have an error which could not have been removed by conjecture during the time interval between those older mss and M - so far as we can judge from the capabilities of the scholars of that period (p.42). Certainly no descriptus will survive this process of selection; but there is a danger that many a ms which does have independent worth will also be discarded, for no better reason than that the text does not offer in its favour any incontestable example of a separative error.

Pasquali, however, advocates (pp. 30 ff) that our initial presumption be that each ms is independent of its predecessors, and he would not believe any ms to be a descriptus in the absence of direct proof. He indicates what might constitute such proof¹; but this policy involves a danger of admitting to consideration descripti which we would have done better to disregard. We may spend much time collating mss which add little to our knowledge of the text (though they may shed some light on scribal psychology and the history of scholarship); and if a ms which is in fact a descriptus but escapes elimination, has been worked over by a humanist scholar and furnished with plausible emendations, we may come to believe these attractive readings to be variants well-rooted in the

1. For example, if one ms has been stained or damaged at certain points in the text, and another has left blank spaces at those same points, we may infer that the latter is derived from the former (p.33).

tradition, when they are in reality conjectures which came in at the last moment. A policy close to Pasquali's is exemplified in the new critical edition of the Old Testament in Syriac, which is to be based on collations of almost all the mss - numbering in some cases sixty or more - extant for each book (apart from the Psalms and Canticles, on account of the sheer abundance of material); the number of mss eliminated as descripti is only two¹.

One might have thought that experience would already have shown which of these two opposed views was generally closer to the truth, and that a study in terms of our probability model was unnecessary. The problem is, however, that either view will yield a self-consistent picture. The higher our estimate of the capabilities of the more recent scribes in emending the text, the readier we shall feel to treat attractive readings in later mss as conjectures, and the more of these later mss will be eliminated as descripti; but the question of the attainments of these scribes is still very much open to debate, and two investigators who disagreed sharply would reach very different conclusions. We therefore need a criterion that is not wholly dependent on the nature of the text and the variants themselves, and we look to our model for some clue.

1. See the List, pp. 56, 66.

We therefore set out to estimate what proportion of the mss extant at time t will have one or more surviving ancestors (and therefore be descriptive). Consider a ms which has j surviving ancestors at time t . In the interval $[t, t+\delta t]$, it will be subject to the following transition probabilities:

- $\lambda \delta t$: creation of new ms, with $(j+1)$ surviving ancestors
- $\mu \delta t$: death of ms
- $\mu j \delta t$: death of one ancestor - our ms will then have only $(j-1)$ surviving ancestors

We now adopt a deterministic approximation of the whole process, from $t=0$ till $t=\tau$. Let n_j denote the number of mss which exist at time t and have exactly j surviving ancestors, and let us put $G(z) = \sum n_j z^j$. Replacing the above transition probabilities by their deterministic counterparts, we find that in $[t, t+\delta t]$,

$$z^j \rightarrow (1 - \mu \delta t) z^j + (\lambda \delta t) z^{j+1} + (\mu j \delta t) (z^{j-1} - z^j)$$

Hence G satisfies the Lagrange linear equation

$$\frac{\partial G}{\partial t} + \mu(z-1) \frac{\partial G}{\partial z} = (\lambda z - \mu) G$$

with subsidiary equations

$$dt = \frac{dz}{\mu(z-1)} = \frac{dG}{G(\lambda z - \mu)}$$

We put $\Gamma = \log G$; the last term then becomes $\frac{d\Gamma}{\lambda z - \mu}$.

By equating the first and second terms, we find one integral of the system to be $u=(z-1) \exp \left[- \int_0^t \mu(\theta) d\theta \right]$. The method of the Jacobi multiplier yields another integral, namely

$$v = L(t) - M(t) + (z-1)e^{-M(t)} \int_0^t \lambda e^{M(\theta)} d\theta - \Gamma,$$

where $L(t) = \int_0^t \lambda(\theta) d\theta$, $M(t) = \int_0^t \mu(\theta) d\theta$.

The general solution of the equation is $\phi(u, v) = 0$, where ϕ is an arbitrary function. To ascertain ϕ , we use the fact that $G=1$ when $t=0$. Substituting these values in our expressions for u and v , we find that, at $t=0$, $u=z-1$ and $v=0$; thus the particular solution $\phi(u, v) = 0$ that we require is simply $v = 0$. Hence

$$\log G = L(t) - M(t) + (z-1)e^{-M(t)} \int_0^t \lambda(\theta) e^{M(\theta)} d\theta.$$

$G(z=1)$ gives the total number of mss at time t , and $G(z=0)$ the total number of independent mss (i.e. not descripti) at that time. The ratio of the latter to the former, when $t = \tau$, is

$$\exp \left[-e^{-M(\tau)} \int_0^\tau \lambda e^{M(\theta)} d\theta \right],$$

which represents the proportion of independent mss among the final population resulting from a deterministic process.

This seemingly unpromising expression is capable of interpretation. Let us define the pre-population of a ms as the set of mss that were in existence immediately before that ms was 'born'; and let us consider the harmonic mean of pre-population size, over all our extant mss. In our

deterministic model, the harmonic mean may be calculated as follows. At any time t , the population is $\exp [L(t)-M(t)]$. In the ensuing time interval $[t, t+\delta t]$, an amount $\lambda(t) \exp [L(t)-M(t)] \delta t$ is added; what survives of that amount at time $t=\tau$ will be $\lambda(t) \exp [L(t)-M(\tau)] \delta t$. Thus, the final population, of size $\exp [L(\tau)-M(\tau)]$, that is extant at time $t=\tau$, can be regarded as the sum of the contributions $\lambda(t) \exp [L(t)-M(\tau)] \delta t$ which are provided by each infinitesimal time interval¹ $[t, t+\delta t]$; every such contribution will have t as its date of origin, and will have pre-population $\exp [L(t)-M(t)]$. The harmonic mean of pre-population size, over all these contributions, is then $1/\eta$, where

$$\eta = \frac{1}{\exp [L(\tau)-M(\tau)]} \int_0^\tau \frac{\lambda(t) \exp [L(t)-M(\tau)] dt}{\exp [L(t)-M(t)]}$$

$$= e^{-L(\tau)} \int_0^\tau \lambda(t) e^{M(t)} dt$$

Now the population at time $t=\tau$ is $e^{L(\tau)-M(\tau)}$. Therefore our expression above for the proportion of independent mss can be identified with $e^{-\xi}$, where

$$\xi = \frac{\text{total number of extant mss}}{\text{harmonic mean of pre-population size of all extant mss}}$$

1. This is not totally accurate. Integration shows that these contributions make up an amount $\{ \exp [L(\tau)-M(\tau)] - \exp [-M(\tau)] \}$ out of the final population $\exp [L(\tau)-M(\tau)]$. The shortfall of $\exp [-M(\tau)]$ represents what is left of the original member; but that amount will be very small - it equals in fact the probability that (in a stochastic process) the original should survive to the present day - and can be neglected.

We shall soon venture to carry over this formula to real traditions, which are of course the product of a stochastic process. Thus we shall hope to evaluate x , where

$$x = \frac{\text{total number of extant mss}}{\text{harmonic mean of pre-population size of all extant mss}}$$

and to regard e^{-x} as an estimate of the proportion of independent mss.

By similar reasoning, we can obtain an estimator for the proportion of independent mss among those extant mss that were written before a given date t_0 . As we have already shewn, that collection of mss can be regarded as the product of a birth-and-death process with birth- and death-rate $\lambda_0(t)$ and $\mu_0(t)$, where $\lambda_0(t)$ equals $\lambda(t)$ for $t < t_0$ and is zero for $t \geq t_0$, while $\mu_0(t) = \mu(t)$ throughout; and we re-work the above calculations accordingly. We then find the required estimator to be e^{-x_0} ,

$$\text{where } x_0 = \frac{\text{total number of extant mss that pre-date } t_0}{\text{harmonic mean of pre-population size of all extant mss that pre-date } t_0}$$

Before trying to apply these results to any real tradition, let us check them against our observations for the fifteen experiments:

TABLE A.6

Experiment no.	Final population	Estimated percentage of independent mss	No. of independent mss	
			Observed	Estimated
1	1	36.8	1	1
2	31	43.4	19	14
3	15	16.3	3	3
4	12	53.4	7	7
5	16	50.7	7	9
6	35	46.4	19	17
7	2	55.8	1	2
8	29	48.8	18	15
9	57	43.7	36	25
10	16	40.5	12	7
11	45	31.8	18	15
12	15	34.5	5	6
13	21	37.7	10	8
14	92	45.0	46	42
15	10	48.2	6	5

In spite of a definite tendency to underestimate the true proportion of independent mss (cf especially the ninth and tenth experiments), our estimator does reflect the changing proportions fairly well.

In real traditions, we know the numerator of x or x_0 , and can form some idea, albeit rather a nebulous one, of the denominator. The fact that we require the harmonic mean, as opposed to the more familiar arithmetic mean, is best neglected; our estimates of the quantities to be averaged out are too vague for the use of one type of mean rather than another to make any appreciable difference. Let us consider for example, the tradition of Aeschylus: Persae, for which statistics are given in Table A.5. We first focus our attention

on those mss that are older than 1300 A.D. There are five of these, and their mean pre-population may be estimated somewhere between 40 and 100. These limits are due to nothing more than guesswork; should the reader feel bound to contest them, I shall, perversely enough, be gratified, because the confidence of his dissent will signify that the mean pre-population, and therefore the proportion of independent mss, can be assessed more accurately than I have anticipated. If this pre-population estimate be granted, then, x_0 will lie between 88% and 95%. On that basis, we shall expect the five mss all to be independent, but would not be shocked to find that one of them was not. We now move on to the period up to 1500 A.D. We have 58 pre-1500 mss, the great majority of which date from the Renaissance; the mean pre-population is now much higher, and may be imagined to have been between 80 and 200; a value between 0.3 and 0.7 results for x_0 ; the proportion of independent mss among these 58, i.e. e^{-x_0} , can then be estimated between 50% and 74%; hence the number of independent mss should lie somewhere between 30 and 40, or not far outside that range.¹

The student of any text which survives in a rich tradition of mss, may readily perform such calculations for himself. Some ingenuity may be demanded of him in estimating

1. Descripti are naturally more frequent among the later mss, but many late mss are nevertheless independent; the reader is invited to refer to fig. A.6 (on p. A:51.) regarding the looseness of the association between age and independence.

the mean pre-population; but no mathematical feat is required, beyond division and reference to a table of the function e^{-x} .

Thus the work of this section has set up between two entities (namely the changing size of ms population, and the proportion of independent mss) which can be reduced in only the vaguest terms to a numerical form, a relationship that could not have been suspected a priori; and the knowledge that the relationship exists will, it is hoped, shed some light on both the imponderables concerned.

§9. Age and generation

It is sometimes maintained that the later mss in a textual tradition are of far less value than the earlier ones, on the grounds that they are removed by a greater number of stages from the original, or, in genealogical terms, that they are of later generation. Certainly there is some truth in this assertion: there must be a 'long-term' correlation between the date of a ms and its generation. What we must ascertain, however, is how this relationship operates. Is it strict enough for us to be confident that two mss of the same period have been through roughly the same number of copies? Could we state that a cent. xv ms has almost certainly been through more copies than a cent. x ms? Or is the association looser, and if so, how loose?

Our model takes us some way towards answering this question. Let us define the original to be of generation zero, a direct copy thereof to be of generation one, and so on. Thus a j -th generation ms will have j ancestors (in which number the original is included); in contrast to §8, we are not concerned here whether these ancestors are alive or dead.

Consider a j -th generation ms extant at time t . It is subject, in time $[t, t + \delta t]$, to the transition probabilities

$\lambda \delta t$: creation of a new ms, of generation $(j+1)$

$\mu \delta t$: death of ms

Once more we use a deterministic approximation. Let the number of mss of generation j that exist at time t be n_j ; we put $G(z) = \sum n_j z^j$. The deterministic analogue of the transition probabilities is that in time $[t, t + \delta t]$,

$$z^j \rightarrow (1 - \mu \delta t) z^j + (\lambda \delta t) z^{j+1}$$

(Thus the symbols n_j and $G(z)$ do not, of course, have the same meaning as in §8.) This leads to the differential

equation $\frac{\partial G}{\partial t} = (\lambda z - \mu) G$; and as $G=1$ when $t=0$, the

required solution is $G = \exp [zL(t) - M(t)]$. As the total population is $\exp [L(t) - M(t)]$, the proportion which is of generation j is $\frac{e^{-L(t)} [L(t)]^j}{j!}$

If it is satisfactory to carry over this result to a stochastic process, then we may surmise that, for mss in existence at time t , generation is Poisson-distributed with

mean $L(t)$. Furthermore, if we suppose that each of the mss in existence at any given time has the same probability of being copied, it will follow that the generation of a ms which originates at time t will be a variate $\Gamma = X+1$, where X is a Poisson variate with mean $L(t)$.

The former of these two tentative results can be checked by means of our artificial traditions. At the end of the process, $L(t) = 13.5$. Pooling all fifteen traditions, we obtain 397 mss, the generation of which is found to range between 4 and 21, the mean value being 12.64. In the following table, they are broken down according to their generations, and compared with the numbers expected according

TABLE A.7.

<u>Generation</u>	<u>Observed</u>	<u>Estimated</u>
6 or less	19	7.6
7	15	8.8
8	18	14.9
9	21	22.3
10	29	30.2
11	40	37.0
12	42	41.6
13	52	43.2
14	35	41.7
15	41	37.5
16	31	31.7
17	21	25.1
18	17	18.9
19	14	13.4
20	1	9.0
21	1	5.8
over 21	0	8.1
	<u>397</u>	<u>396.8</u>

to the above theory. If we compare the observed and estimated figures by eye, we find tolerable agreement, though there is

considerable discrepancy at the two extremes.

Let us therefore follow up the possibility of utilising the second result in order to estimate within real traditions the generation of mss of a given date. We shall need to assign a value to $L(t)$; but so far we have not succeeded in estimating $\bar{\lambda}$ except over periods for which we have extant mss. If our mss are all many centuries later than the original, it will be impossible to estimate $L(t)$ with any confidence. In the case of the Persae, for example, we have 5 mss earlier than 1300 A.D., and 63 earlier than 1600; we thence arrive at an estimate $\bar{\lambda} = (\log 63 - \log 5)/300 = 0.0084$ for that three-century period; but we can only guess at $\bar{\lambda}$ over the previous eighteen-odd centuries, from the composition of the work onward (though it seems on intuitive grounds unlikely that $L(t)$ will in any case be more than 20, say). In some instances, however, the outlook is more encouraging. For the Bede tradition, we have 15 mss earlier than 900, and 155 earlier than 1600, so that $\bar{\lambda}$ is estimated at 0.0048 over that interval; as the work was composed in 731 A.D., we can hardly go far astray if we extend this value of $\bar{\lambda}$ to the period before 900; hence we may estimate $L(t)$ for any time we choose, and set up a confidence interval for the generation of a ms copied at that point. On that basis¹ we may estimate, for example, that the generation of a ms copied in 1500 A.D. will lie between 2 and 8.

1. What follows is a 95% confidence interval derived from a Poisson distribution with mean $L(1500 \text{ A.D.})$.

At all events, if the distribution of generation is approximated, even very roughly, by a Poisson distribution of which the mean lies within the range here envisaged, then, in view of the extent to which observations on such a Poisson distribution are known to fluctuate on either side of the mean, we may conclude that the association between age and generation will not be at all strict. Two aspects may be noted. First, mss dating from a single period may be expected to vary greatly in respect of generation; we ought not to be surprised if, of two contemporary mss, one has passed through one and a half times as many intermediate copies than the other, and presents a correspondingly more corrupt text. In the eighth experiment, for example, the breakdown of mss by generation is:

TABLE A.8.

Generation	8	9	11	12	13	14	15	17	18	19	20	21
No. of mss	2	2	2	2	4	3	2	4	3	3	1	1

If further copies were made when the tradition was thus constituted, then such new mss could be of almost any generation from 9 to 22, and yet go back to the same period.

The second point is that the fact that one ms is older than another, even by several centuries, is no guarantee that it is of lower generation. Let us consider, for example, two mss of the Persae, one dated ca. 1300, the other ca. 1600. Certainly the mean value of generation for mss of the former period will be less than for the latter, by about 3; for

$L(1600) - L(1300) = \log 63 - \log 5 = 2.53$. Other traditions yield similar figures:

TABLE A.9

<u>Tradition</u>	<u>Period considered:</u>		<u>Estimated difference</u>
	<u>from</u>	<u>to</u>	<u>in mean generation</u>
Sophocles: Ajax	1300	1600	3.17
Aristophanes: Clouds	1300	1600	3.10
Cyprian: De Unitate	900	1600	2.29
Bede: Eccles. Hist.	800	1600	3.84
Peshitta version of Genesis	700	1900	2.34

These amounts, however, are all dwarfed by the large fluctuations, illustrated in Table A.8, which will be found among contemporary mss of either period. We should not therefore be surprised to find that one of our oldest mss is removed from the original by more intermediate copies than a ms which is several centuries younger.

This last point is amply borne out by our simulated traditions. In Fig. A.6, generation is plotted against date of origin for the 35 mss which survive at the end of the sixth experiment. There is a noticeable association between the two, which is however reduced considerably if descripti are disregarded. Nevertheless, there are many cases which run against that trend; particularly striking is the production at $t=995$ of a ms which is of the same generation as our oldest ms, written at time $t=642$. We may conclude that when dealing with real traditions too, we could not be justified in disparaging the later mss

KEY: ● INDEPENDENT MS. ○ DESCRIPTUS

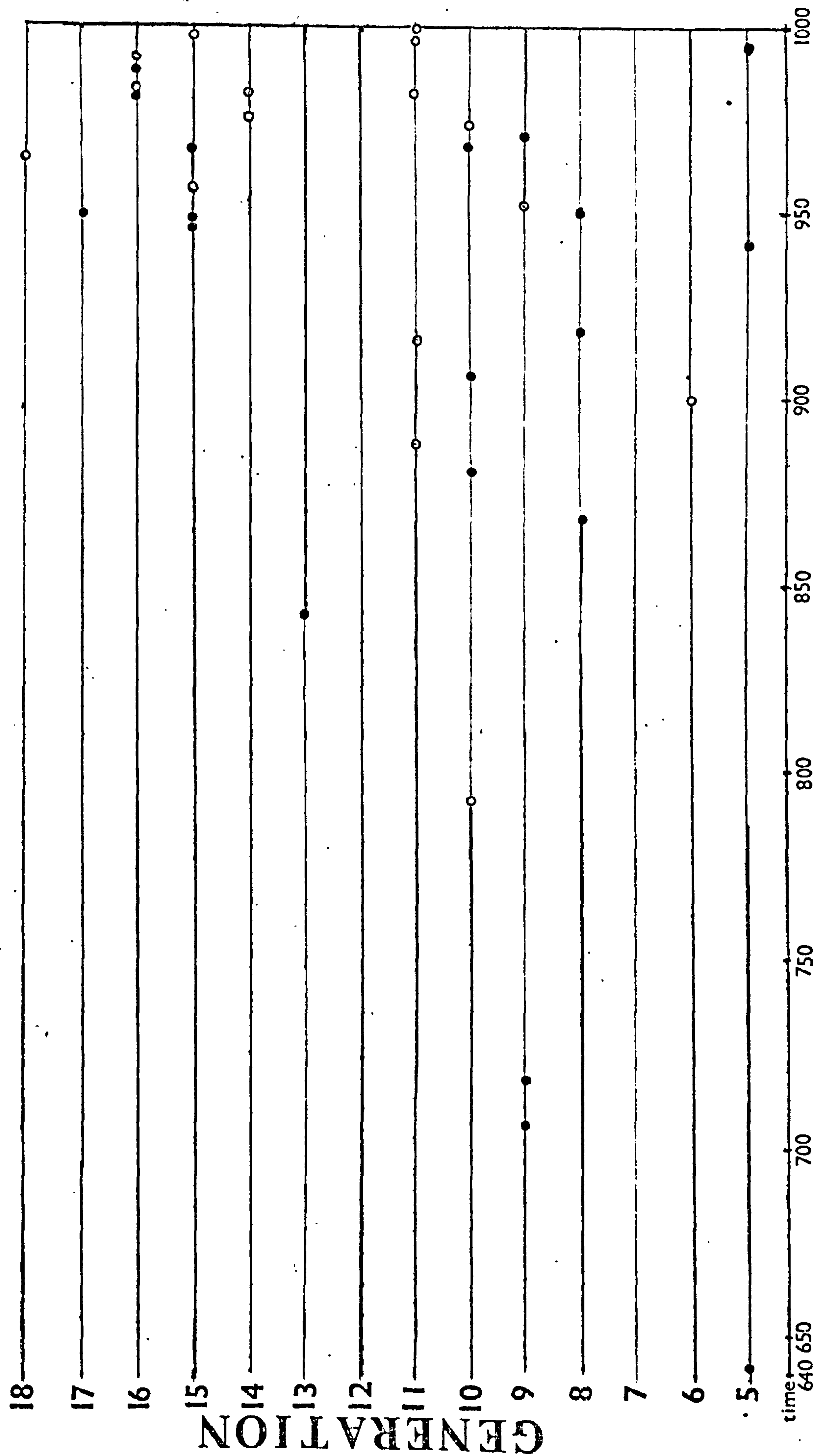


FIG. A.6

on the pretext that they are necessarily farther removed from the original.

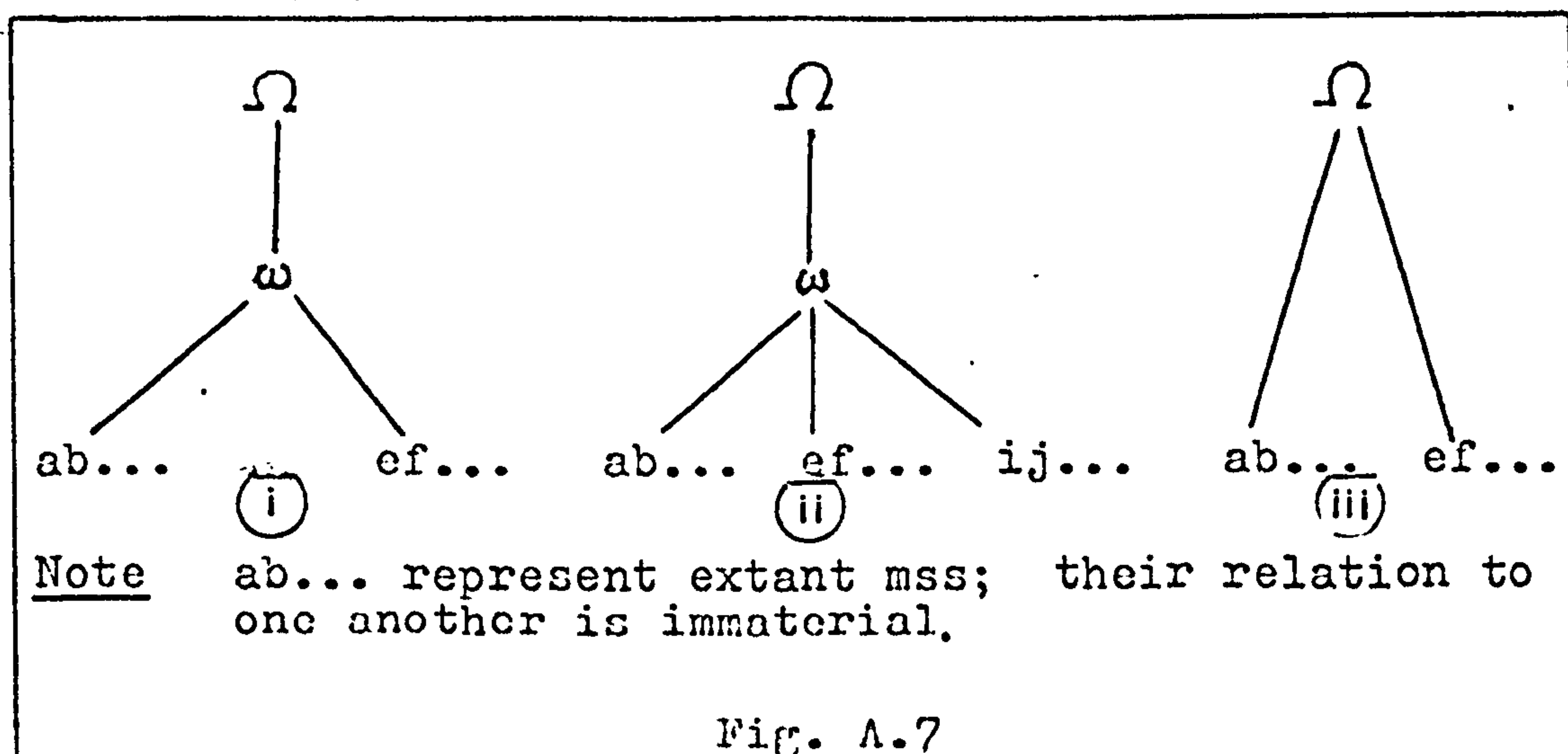
§10. The archetype

We now turn our attention to the latest common ancestor of all the surviving mss, termed the archetype. In the Maasian system, this is the earliest text which the extant tradition will enable us to reconstruct. The accepted doctrine moreover posits that the archetype will be distinct from, and thus later than, the original, and will therefore have lost the original reading at certain points; hence there will, in general, be some scope for emendation, even though the tradition may embody a great number of mss.

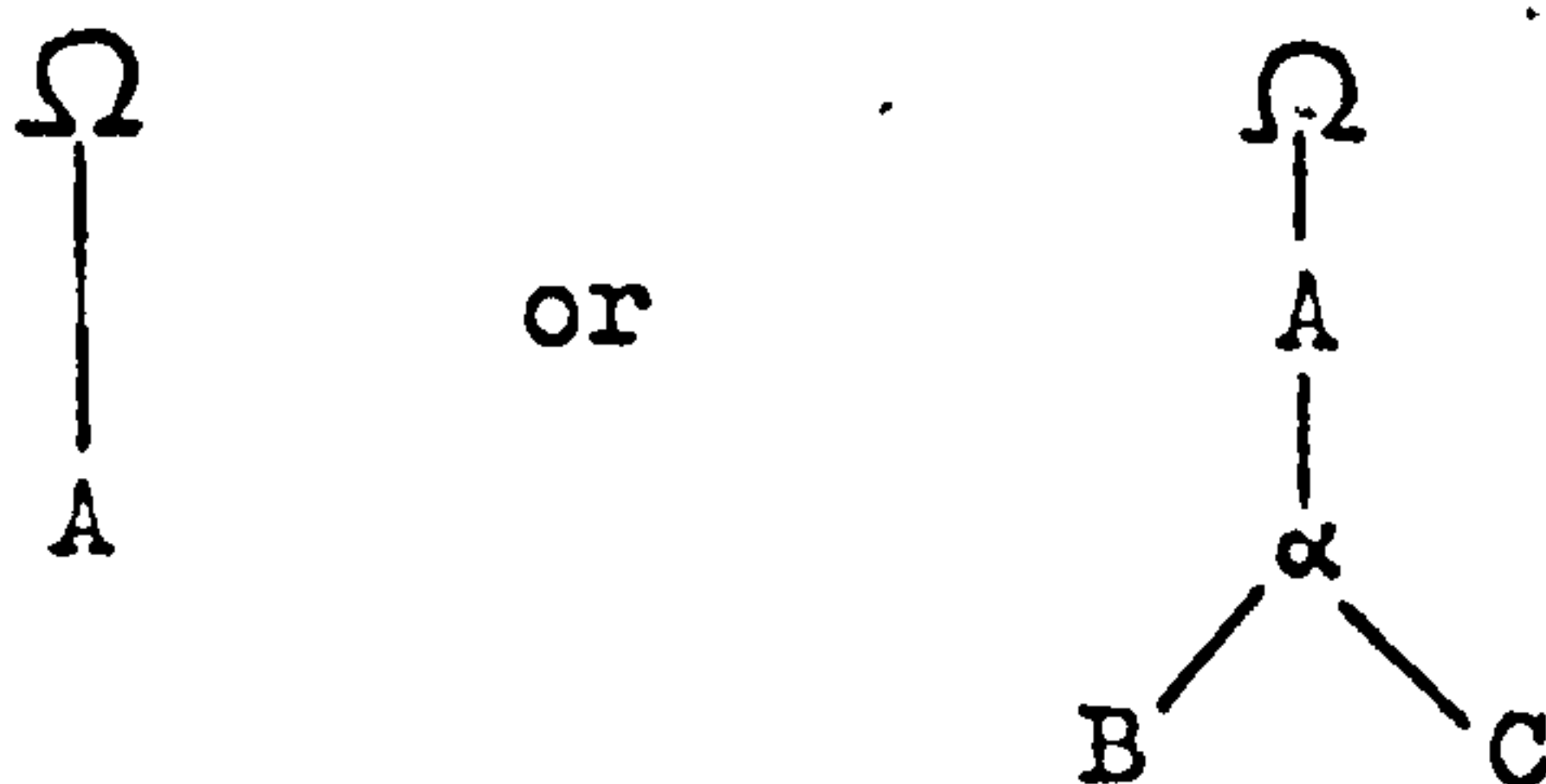
In each of the fifteen simulated traditions, it was found that all the extant mss could indeed be traced back to a common ancestor later than the original - even when the final population was as high as 92. We may wonder why it should apparently be normal for the extant mss all to be dependent on an archetype tainted with errors, instead of going back directly to the original; and we may ask, with Pasquali¹: "Was there always an archetype [sc. distinct from the original]?"

1. Title of Ch. II ("Ci fu sempre un archetipo?")

It is helpful to define the idea of traces of a ms, as follows. A ms will be said to leave n traces, if there are n direct copies made from it which today either survive or leave extant progeny. The archetype, which is by definition a branch-point in the stemma, may be defined as the earliest ms to leave two or more traces¹. The formulation of Maas teaches that the original will normally leave one trace only, and that a case like fig.A.7(iii), where the original has left more than one trace and is therefore itself the latest common ancestor of the extant mss, is exceptional. Let us now try to explain why it is so rare for the original to leave more than one trace.



1. The only possible case in which this definition would be at fault is that where only one ms, with or without a number of 'descripti', survives, and therefore deserves the title of archetype, such as



We consider a much simplified model, which nevertheless gives us a lead. Suppose a stochastic process with constant birth-rate λ and death-rate μ ($\lambda > \mu$), and consider the outcome as $t \rightarrow \infty$. Any given ms has probability μ/λ of leaving no trace eventually, and hence $(\lambda - \mu)/\lambda$ of leaving one or more traces. What is the distribution of T , the number of traces left by the original? If we assume that the original dies at time $t=t_0$, then the number of copies made from it is a Poisson variate with mean λt_0 , and the number of traces ultimately left by it is a Poisson variate with mean $(\lambda - \mu)t_0$. But the probability of the original dying in the time interval $[t, t + \delta t]$ is in fact $\mu e^{-\lambda t} \delta t$; wherefore the distribution of T is given by the p.g.f. $G(z)$, where

$$G(z) = \int_0^{\infty} \mu e^{-\lambda t} e^{(\lambda - \mu)t(z-1)} dt = \frac{\mu}{\lambda - (\lambda - \mu)z}$$

$$= \frac{\mu}{\lambda} \sum_{j=0}^{\infty} \left(\frac{\lambda - \mu}{\lambda} \right)^j z^j$$

Thus the probability that the original leaves no trace is - as we already knew- μ/λ ; and the probability that it leaves one trace is $\mu(\lambda - \mu)/\lambda^2$. Thus the probability that the original leaves just one trace, when it is known that the tradition does not become extinct, is

$$\frac{\Pr(T=1)}{\Pr(T>0)} = \frac{\mu}{\lambda},$$

an expression which is identical with the a priori probability that a tradition transmitted under the same conditions should become extinct.

But that probability is exceedingly high. We have already remarked that by far the greater part of classical literature has been lost. Of relevance also is the well-known study of A.J. Lotka, on the extinction of family surnames¹. Using data for the white population of the U.S.A. in 1920, Lotka found that the probability of any given male line ultimately becoming extinct was about 88%. Now there can be little doubt that, in respect of the ability to reproduce after one's own kind, the classical manuscript is no match for the U.S. white male; hence the a priori probability of extinction for a textual tradition in antiquity must in most cases have been rather more than 90%. We can therefore understand why it happens so seldom that the only common ancestor of all the extant mss is the original. Some considerable good fortune is required for the original to leave a trace at all; it is only to be expected that among textual traditions that survive (i.e. where the original has left at least one trace), the great majority have an archetype distinct from the original (i.e. the original has left only one trace).

1. "The extinction of families", in Journal of the Washington Academy of Sciences (1931), pp. 377-380, 453-459.

There are, of course, a few exceptions, some of which have been pointed out by Pasquali (pp. 15 ff). It is noteworthy that for two of these (the Vulgate Octateuch and Virgil: Aeneid), the a priori chance of survival must have been far greater than for almost any other ms tradition.

Thus the emergence of an archetype is a natural and well-nigh inevitable side-effect of the birth-and-death process whereby a textual tradition evolves. Special explanations, of the sort contemplated in R-W (p. 52), are unnecessary. There is no need to suppose, for example, that the book survived the dark ages in one copy only - though that may indeed have sometimes been the case. Nor do we have to adopt the theory of Maas¹, that when the scholars of cent. ix changed over from uncial to miniscule script, one uncial ms was transliterated and laboriously furnished with the appropriate breathings and accents, and that as the demanding task of transliteration would not have been undertaken more often than was absolutely necessary, this first miniscule copy served as the source for all subsequent copies. It is just as well that we are not forced to appeal to Maas' hypothesis, because there is considerable evidence that in some Greek traditions the archetype must be dated much earlier than cent. ix (R-W, loc. cit.).

1. which may be conveniently found set out in Pasquali's Italian translation, appended to his Storia (pp. 487 ff), of Maas' "Schicksale der antiken Literatur in Byzanz".

Having dealt with the existence of the archetype, we now turn to the question of its date. The matter is not easily settled by analytic methods; however, we obtain some clues by identifying the archetypes in our simulated traditions.

It seemed possible that the date of the archetype was related to the fluctuations in population size over the years. Accordingly, fig. A.8 records, for two of our simulated traditions (the third and the tenth), the changing size of population over the course of the experiment, and also indicates the life-span of the archetype. In both cases, it will be seen that at some time during that life-span (early on, in the latter experiment; rather later, in the former), the population was at a very low level, from which it recovered and to which it never fell again. That virtually the same can be said of all our fifteen traditions, will be apparent from Table A.10: the amount in the eighth column is nearly always smaller than that in the ninth (though experiment no. 13 is an exception).

Concerning every one of these traditions, one could say (a little whimsically) that it grew and decreased fitfully for a while, as if unable to decide whether to survive or not, and then took a firm step in the direction of growth and never really turned back. In these experiments, then, the archetype can be associated with the last period of crisis, just before the population proceeded to flourish; by 'associated' it is meant that the most recent phase during which the very survival of the tradition was

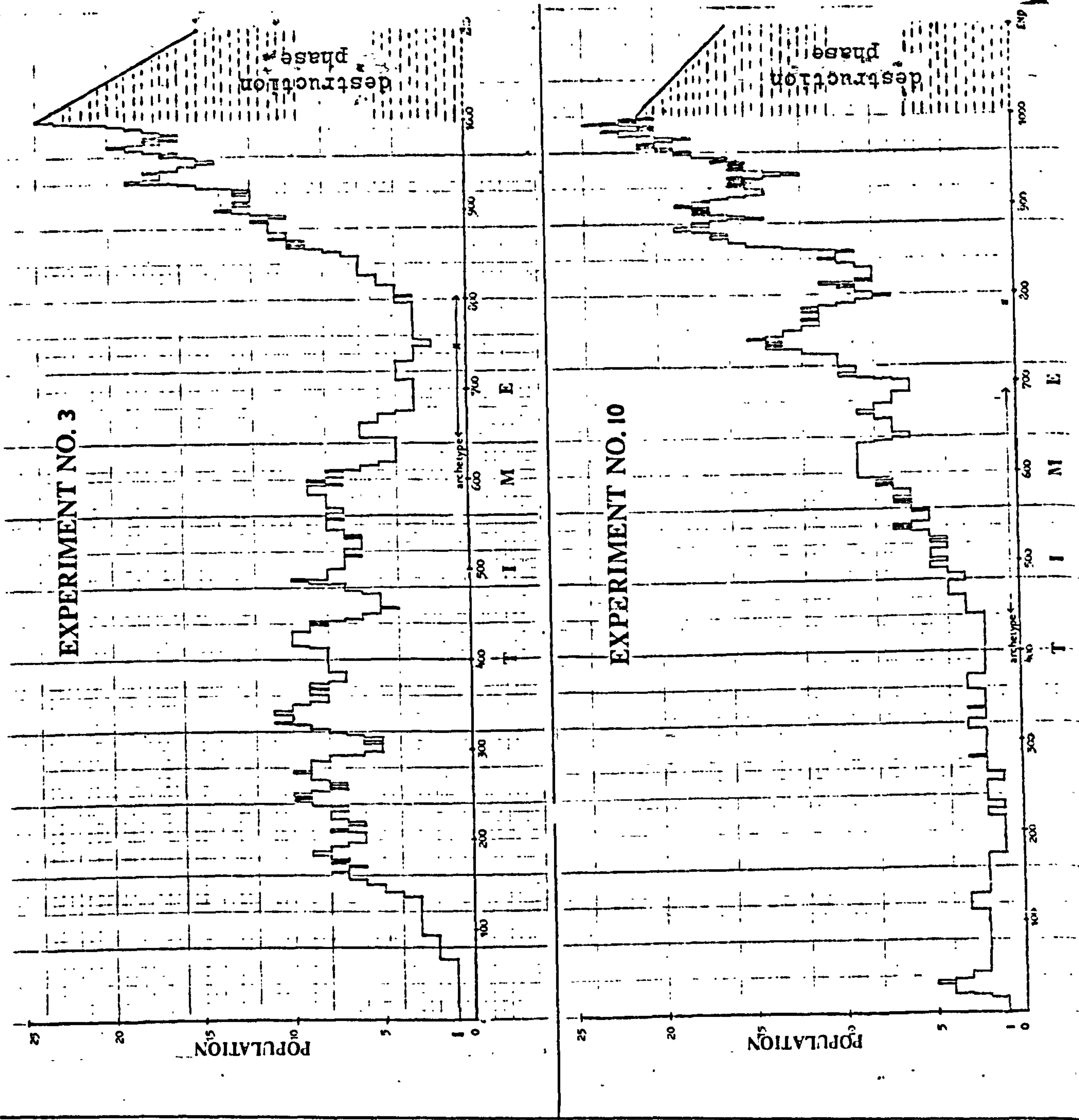


FIG. A.8

EXPLANATION The life-time of the archetype is indicated by a line between arrows (thus) and the date of accession by a cross.

TABLE A.10

Experiment number	A R C H E T Y P E					POPULATION		
	D a t e o f :		Accession	Life-span	Generation	Number of branches	Minimum during after life of archetype	Final
	Birth	Death						
1	612	α	681	388 γ	4	α	α	1
2	414	650	631	236	6	2	20	31
3	648	802	747	154	8	2	3	15
4	223	395	254	172	1	2	9	12
5	53	574	210	522	2	2	7	16
6	110	289	178	179	1	2	3	35
7	967	α	β	33 γ	13	α	α	2
8	39	288	132	249	1	3	4	29
9	26	384	66	358	1	3	5	57
10	441	690	787	249	3	2	6	16
11	375	632	462	257	7	2	7	45
12	291	647	328	356	1	2	4	15
13	105	203	244	98	1	2	3	21
14	60	226	133	166	2	3	8	92
15	526	805	650	279	7	2	8	10

Note. All dates etc. have been rounded to the nearest integer.
Key to symbols. α : inapplicable, in that the archetype is still extant
 β : during the destruction phase (after t=1000)
 γ : and still 'alive'

in jeopardy, occurred at some time during - or shortly before or after - the life-time of the archetype.

The explanation of this result is not far to seek. In general the archetype is, by definition, the earliest ms to leave two or more traces. Now if we consider a ms which perished long before the last crisis period, we may doubt whether two lines of descent proceeding from it could both have survived the crisis. On the other hand, if we think of a ms which did not come into existence before the recovery was well under way, we can hardly believe that no earlier or contemporary ms gave rise to a line of descent surviving to the end of the process, and hence that the whole extant tradition derives from that one comparatively late ms. Thus we can understand why the last crisis phase is a period in which the archetype is likely to have been in existence; and this line of argument can be applied to real as well as to artificial traditions. The textual critic will often be able to identify the most recent period of crisis, on the basis of his information regarding the history of scholarship; and it is with that period that the archetype can with some confidence be associated.

Another point apparent from Table A.10 is the longevity of the archetype. In these experiments, the life-time of a ms is exponentially distributed, as we have said; $\mu=0.0115$ and the expected life-span is therefore about 87 years. Yet all the archetypes (except no. 7, which is still 'alive') enjoyed a far longer existence; admittedly, the exponential distribution has a long 'tail', but no less than five of the

fifteen archetypes fall within the top 5% of the distribution. This longevity has a bearing on our last result: if we were to suppose that the archetype was in existence during the most recent crisis, the possibility should not be neglected that it was written many years earlier.

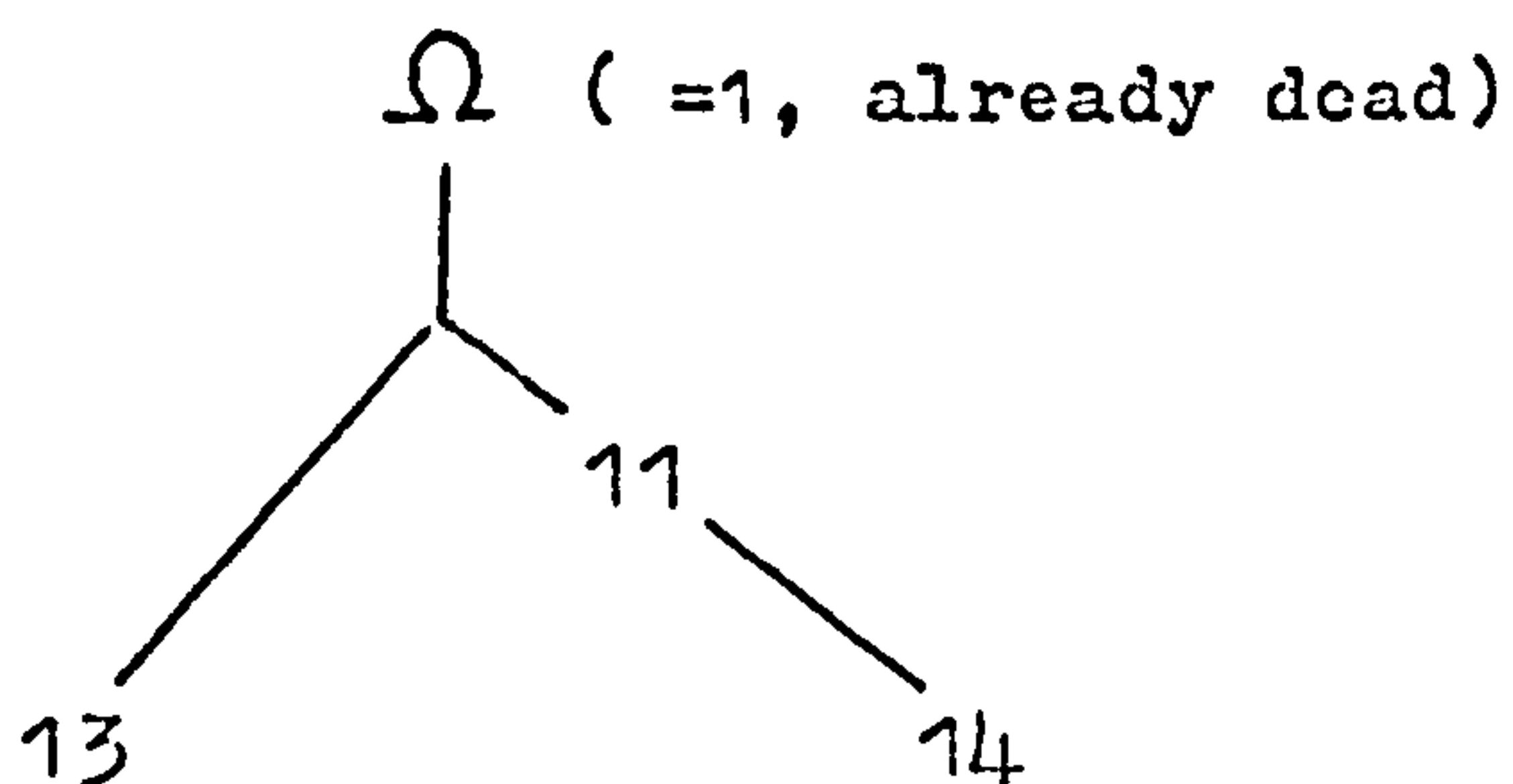
In addition to birth and death, there is a third event which must be considered in relation to the archetype, namely the time at which the last of the mss not derived from it died out; for not until then did it in fact become the archetype. The date of this event - which we have termed "accession" - is shown in the fourth column of Table A.10. Accession occurred at some point between birth and death in most of the experiments; but in nos. 10 and 13, the archetype did not become archetype until its lifetime was over.

The question of accession is relevant to the last point to be discussed in this section, namely the concept of an archetype in a contaminated tradition (i.e a tradition in which scribes frequently utilised more than one exemplar). We find that the effects of contamination vary considerably according to the manner in which the archetype has reproduced at the time of its accession.

At one extreme, the archetype may have acceded by becoming, at some stage in its life-time, the only ms in existence. In that case, no matter how eagerly subsequent scholars consult other mss and contaminate their copies, they will never transmit (though they may through conjecture

hit upon) an ancient reading which was not present in such an archetype¹. When the mss diverge, we shall expect to find the better reading among a reasonably large proportion of the mss, since the purpose of contamination was to diffuse the best text that could be obtained².

A very different prospect is presented by the archetype of the tenth experiment. As in fig. A.2, we denote each ms by a number (1 being the original, 2 the next ms to be produced, and so on). The eventual archetype is no. 14. When it is born, there are three extant mss:



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1. Occasionally, however, an ancient reading may survive in another source (e.g. in a quotation by another author, whose work is soundly preserved at that point).
 2. Sometimes this purpose was not achieved, in that the original reading was ousted through contamination in favour of a reading that was simpler, or more delicate, etc. The true reading may then come to be preserved in a small minority of the mss; but such passages will surely be exceptional.

Thus two other mss, both containing some ancient readings that were lost in the transcription of 14, are in circulation, and liable to be used as subsidiary sources; this is in the year 441. By the time $t=500$, ms 11 is 'dead', and 14 has reproduced. Ms 13, however, is still alive, and has begun to produce a family of its own:



This family descended from ms 13 never consists of more than three mss at any one time, but it does not die out until $t=787$. If we now draw the stemma as it finally appears, we may insert a dotted line such that all mss above it were 'born' at a time when there were still in circulation representatives of another line of descent, which contained sound readings lacking in the progeny of 14, in places where the scribe of 14 (or one of its ancestors) went astray. The stemma is presented thus in Fig. A.9.

Now if the scribe of any ms above the dotted line consulted a ms of the family issuing from ms 13, and introduced some of its readings into his own copy¹, then we shall find ancient readings preserved sporadically in

1. He may have adopted it for the text, making no mention of the reading of his main source; or he may have merely recorded it in the margin, tempting whoever might copy the ms to introduce it into the text itself.

isolated branches of the tradition, or even in one sole ms - a phenomenon which has indeed been reported in various contaminated traditions¹. It should not be forgotten, however, that even in this situation there will remain a number of passages where the archetype had an error which has not been corrected away in any of our extant mss.

Our fifteen experiments present, in addition to the two sharply contrasted structures described in the last two paragraphs, various intermediate cases; and it is not unlikely that contaminated traditions of real texts will also be distributed quite widely over this spectrum.

In all such traditions, apart from those which lie at our former extreme (i.e. in which the archetype was at one stage the sole survivor), the archetype will play a lesser role than in a pure "Maasian" situation. The extant mss will no longer be derived from it exclusively; it will be no more than a principal source, on which all our extant mss depend to some extent, while they will also contain material which goes back ultimately to a small² number of

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1. e.g. in Homer (Pasquali, p. 211) and Aeschylus (R.D.Dawe, "The Collation and Investigation of Manuscripts of Aeschylus", Cambridge 1964, Ch. V).
 2. Dawe (op. cit., p.159) notes with disapproval a tendency to suppose that there was no more than one extra-stemmatic source: "If we know that extra-stemmatic evidence was available, but are not more precisely informed as to its exact nature, we cannot make any pronouncements about the number of such sources, and there is no more merit in postulating one than ten thousand". Though I would grant Dawe's main point, that there is no absolutely compelling reason for supposing one such source only, our work above suggests that the mss which were in existence between the birth and the accession of the archetype, and which might have served as extra-stemmatic sources, are unlikely to have been many.

other (sometimes termed "extra-stemmatic") sources. In such a case, there will no longer be much point in re-constituting the text of the archetype as a preliminary step towards the reconstruction of the original.

Nevertheless, the idea that all our surviving mss depend on a certain main source later than the original¹, will always be of some value, if only as an explanation for the errors that are common to the whole extant tradition; I should be sorry ever to renounce, with Ed. Schwartz², the very concept as worthless, or, with B.A. van Groningen³, to suspend the use of the term 'archetype' altogether.

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1. a source which, according to Pasquali (p. 140), can still be referred to as the archetype, "purchè tal concetto sia inteso cum grano salis"; elsewhere, however, he is inclined to prefer the term "capostipite".
 2. "Eusebius' Werke", Ed. ii, Teil iii (Leipzig 1909), p. cxlvi ("Was nützt es einen Archetypus zu fingieren...?")
 3. "Traité d'histoire et de critique des textes grecs", Amsterdam 1963, p. 110 ("Pour ma part, je serais assez porté à mettre au ban pour un certain temps le terme d'archétype et me borner, le cas échéant, à parler d'ancêtre commun d'un groupe de manuscrits").

§11. The two-branched stemma

We have already mentioned (p. A:2) Bédier's discovery that the great majority of stemmata reconstructed by editors of texts were two-branched, and his conclusion that the stemmatic method which had produced this preponderance of bifid stemmata must be at fault. There can be little doubt that the discredit into which 'Maasian' criticism has fallen in many quarters, is due in part to this argument of Bédier.

That 'orthodox' stemmatic procedure rests on a number of questionable assumptions, has been amply demonstrated, notably by Pasquali. The question that we seek to answer here, however, is whether the frequency with which scholars have postulated a two-branched stemma should in itself be taken as an indication that the method is not to be trusted at all, or whether it corresponds to what we may with good reason believe to be the true position. That question has stimulated more controversy and research than can be properly surveyed here; our own discussion will be limited to those aspects of the problem which lend themselves to mathematical treatment¹. We shall first examine the contributions of others, and then (pp. A:76 ff) put forward our own.

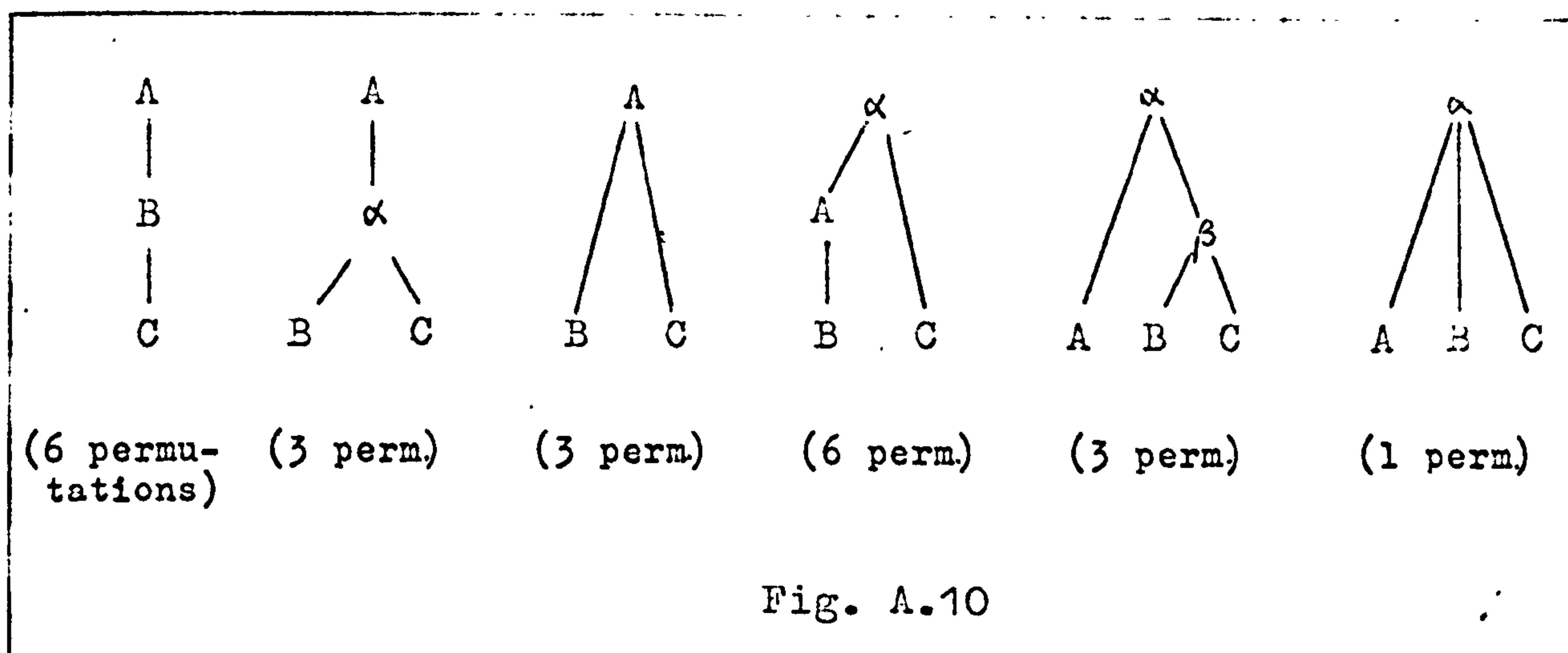
1. For a more general discussion, see the appendix entitled: "Stemmi bipartiti e contaminazione", in S. Timpanaro, "La genesi del metodo del Lachmann", Florence 1963, pp. 112-135.

Our first task is to determine the extent to which the two-branched stemma in fact predominates. Bédier himself claimed¹ to have observed, in the field of mediæval French texts, 105 two-branched stemmata out of 110 (i.e. 95%); he did not, however, publish a list of the works on which that result was based. A careful check by A. Castellani² on the material which may be presumed to have been available to Bédier, suggests that a figure in the region of 80% would have been more appropriate. Timpanaro comes to a similar conclusion (pp. 123 ff) - on the basis of a survey which was of necessity far from exhaustive - with regard to classical traditions: there are a fair number of stemmata with three or more branches, but the great majority have no more than two. Thus the phenomenon pointed out by Bédier, though it may not be quite as marked as he claimed, is nevertheless real enough.

We may begin by considering an argument due to Maas himself. He shows (p. 48) that if we are given three extant mss A, B, C, the number of possible stemmata into which they can be disposed is twenty-two:

1. Romania (1928), p. 171.

2. "Bédier avail-il raison?", Fribourg (Suisse) 1957, pp. 20 ff.

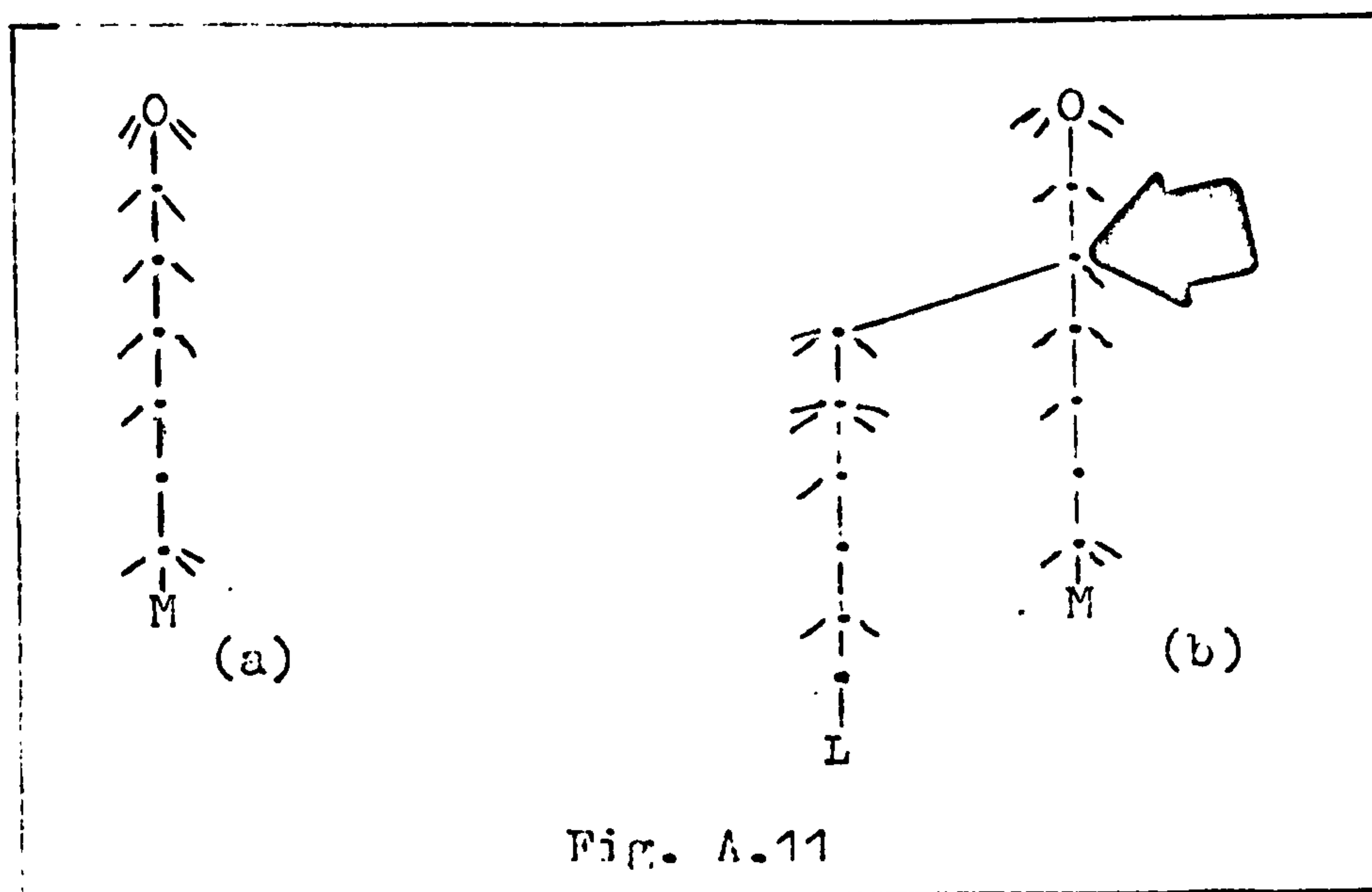


Only one of these twenty-two possible stemmata has more than two branches; and this fact, according to Maas, will account for the rarity of stemmata having more than two branches¹. The fallacy, of course², which has been perceived by many investigators, is that to enumerate all the possible outcomes is only the first step in the solution of such a problem; one must then calculate the probability of each outcome, and there is no reason here for declaring forthwith that all these probabilities are equal. Nevertheless, a few scholars have applauded Maas' reasoning³, and one has even carried it further: W. Hering⁴ has utilised combinatorial analysis to calculate the number of possible stemmata into which one can arrange four,

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1. The proportions 5/110 (Bédier) and 1/22 (Maas) in fact correspond exactly.
 2. I must apologise to the mathematical reader, who may be infuriated by the patently fallacious character of this and other treatments; but I hope that he will find them of interest as specimens of argumentation offered, in relation to a mathematical problem of some difficulty, by philologists of undoubted ability whose mathematics however lies decades behind them.
 3. For references, see p. 116n. in Timpanaro (who is not himself included in that company).
 4. "Zweispaltige stemmata", in *Philologus* (1967), pp. 170-185.

five or six extant mss, to ascertain how many of these have more than two branches, and thereby to estimate what proportion of stemmata having more than two branches one would expect to encounter in practice.

A very different argument was put forward by J. Fourquet¹. He reminds us that the stemma does not exhibit all the intermediate copies between the lost original and the extant mss. Thus, if we indicate a lost copy by a dot and the original by O, and moreover use a line to denote each occasion on which any of these mss was copied, then the ancestry of a typical ms, say a seventh-generation ms M, might be represented as in fig. A.11a. Let us now imagine a second



1. "Le paradoxe de Bédier", in Publ. de la Faculté des Lettres de l'Univ. de Strasbourg, Fasc. 105: Mélanges 1945, II (Etudes Littéraires) - published in Paris, 1946 - pp. 1-16.

extant ms L, which we shall suppose to be of the ninth generation. The path leading down from O to L may branch off from that leading to M at any one of six points shown in (a). Suppose for example that the divergence occurs at the second-generation copy, as in (b). If we now suppose a third extant ms N, then there will be ten possible points from which the path from O to N may branch off from the lines shown in (b). For only one of those ten possible points of departure (indicated by an arrow) will a three-branched stemma result; otherwise, the stemma will be two-branched. Hence the probability that three mss arrange themselves in a two-branched stemma might be estimated, on the basis of this example, as 9/10; and we may expect in general to observe a great majority of two-branched stemmata.

This reasoning was refuted by F. Whitehead and C.E. Pickford¹: If we suppose that there are ten possible points of departure for the line leading down to N, we cannot deduce forthwith that all these ten possibilities are equally probable: for all we know, the one which would result in a three-branched stemma has a greater probability than all the others put together.

Castellani criticises Fourquet on different grounds: In drawing our line from O to M, we must think not only of M but also of the progeny of all the lost intermediaries. Thus, he argues,

1. "The two-branch stemma", in Bulletin bibliographique de la Société Internationale Arthurienne (1951), pp. 83-90.

M's immediate ancestor will have 4 descendants; M's 'great-grandfather' will therefore have twelve (six from either copy); and continuing upwards, we shall find that the original gave rise to a total population of 1820 mss. It seems most improbable that 1820 copies should have been made of any text, especially if only three survive; and Fourquet's argument is thereby invalidated. Castellani's point has been accepted by at least one subsequent investigator¹, but undeservedly. The number that Castellani invites us to consider, and which he could not believe to be as great as 1820, is not, as he asserts, the number of mss which were ever produced by some birth-and-death process (to be called P_1) which might represent the manner in which a particular text was transmitted; it is, instead, the total number which would have resulted from a hypothetical pure birth process, which had the same birth-rate as P_1 but did not suffer any death-component. This latter hypothesis bears no relation to the realities of textual transmission, and the total to which it would have given rise could in fact be far greater than 1820. In terms of our stochastic model, the expected value is $\exp [L(t)]$, which, for our artificial traditions, approaches one million.

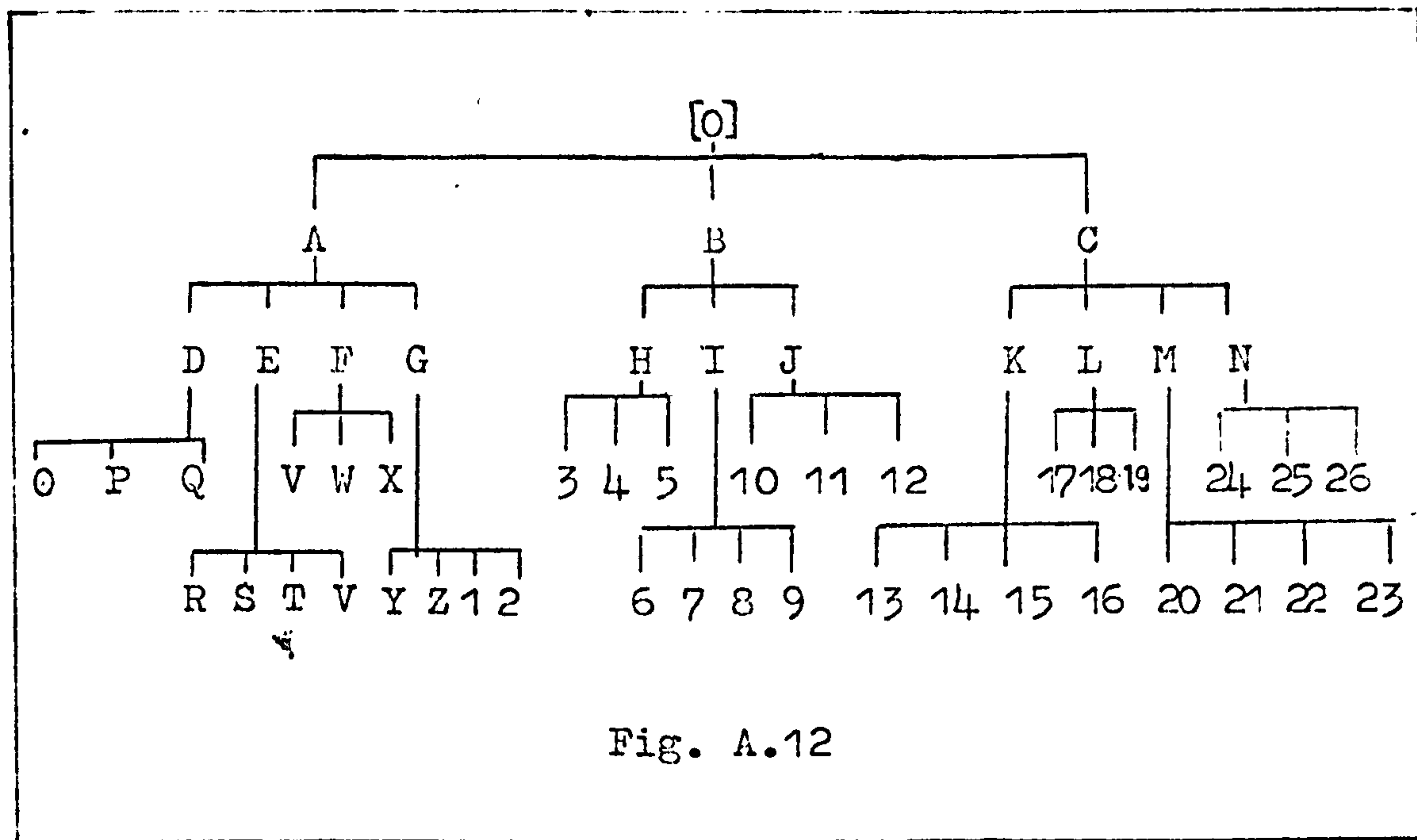
Both Castellani and Whitehead-Pickford suggested solutions of their own. They both adopt the idea that our present collection of mss may be regarded as a remnant of the collection of all the mss that have ever existed. We may follow Castellani in referring to the tree which would express the inter-relations of this larger collection, as the 'arbre réel'.² In both studies it was assumed that all the mss contained in the 'arbre réel' had the same chance

1. namely A. Kleinlogel, "Das Stemma problem", in *Philologus* (1968), pp. 63-82; see p. 69.

2. An example of an 'arbre réel' is fig. A.2.

of survival; hypotheses were set up concerning the family structure of the 'arbre réel'; and an attempt was made to see whether the extant mss, extracted from the 'arbre réel', would fall into only two families (yielding a bifid stemma) or more.

Thus Castellani set up a three-branched 'arbre réel' (fig. A.12), consisting of fifty-two mss. He sought to ascertain whether a two- or a three-branched stemma was more likely to result if n mss were selected at random out of the 52;



various values for n were tried. Both his experiments¹ and his calculations showed that if n exceeded 4, the stemma was more likely to have three branches than two - a result which, taken on its own, would support Bédier's view that the preponderance of bifid stemmata postulated by textual critics is indeed suspicious.

1. performed by means of a pack of cards - hence the number of 52 mss.

Whitehead-Pickford consider 'arbres réels' consisting of either three or four original families, of various proportionate sizes (e.g. 1:1:1, 1:1:2, 1:1:1:1), and calculate the probability that n mss (where $n=3, 4$ or 5) drawn from an 'arbre réel' which has a given family structure, will represent one, two, different families. "This problem is strictly analogous to the well-known one of the marbles: given three bags containing respectively a , b and c marbles, what is the probability that any three marbles taken at random will come from, (a) the same bag, (b) two different bags or (c) three different bags?" (p. 87). They found that the conditions which favoured a bifid stemma were a small number of extant mss, a small number of original families, and disparity in the respective sizes of those families. If the number of extant mss was five or more, the bifid stemma would not predominate except in cases of very marked asymmetry between the families (such as 1:1:5 or 5:5:1) - a result which, broadly speaking, accords with Castellani's.

The arguments of Castellani and of Whitehead-Pickford are both open to serious objection. They regard the extant mss as a random selection from the 'arbre réel', in which every ms has the same chance of survival. This involves a tacit assumption that the history of the tradition is to be divided into two phases: first, a pure birth process, in which no individual died, and which culminated in the

1. performed by means of a pack of cards - hence the number of 52 mss.

production of the 'arbre réel'; and then, a pure death process, in which all the mss were equally at risk, and the 'arbre réel' was whittled down to the set of mss extant today. But such a separation of the processes of birth and death is completely unrealistic; the two went on, side by side, right down the centuries.

A. Kleinlogel has pointed out (p. 73) that the fact that the set of extant mss cannot be regarded as a random selection from the 'arbre réel', will be self-evident if the question of age is considered. The average age of a random sample drawn from the 'arbre réel', being close to the average age today of all the mss (whether alive or dead) that have ever existed, will exceed by far the average age of the extant mss alone. He therefore suggests that Castellani's model be modified, the chance of survival being made an appropriate function of the date of origin of a ms, instead of being equal for all the mss. But the fallacy inherent in regarding the extant mss as being derived by some process from the 'arbre réel', and thus in setting up a textual history which consists of two successive and wholly separate phases, of birth and of death, still remains, whether or not different mss are assigned different chances of survival in the death phase. True, a set of mss arrived at in this way will not be markedly different in age structure from the set of extant mss, for age structure has been expressly taken into account; but we have no guarantee that it will not be different in other respects, including the number of branches of the stemma.

The 'arbre réel', therefore, ought not to be the starting-point of our analysis. We must treat the birth and death components as simultaneous; and a solution on those lines can be proposed in terms of our stochastic model.

As we stated on p. A:53, an archetype must by definition leave at least two traces (i.e. at least two direct copies thereof must have survived or have left descendants). Bédier's complaint was that in the great majority of cases, no more than two copies were found to have left a trace. Posed in this way, the question closely resembles our earlier problem regarding original and archetype, and can be treated similarly. We recall that for a stochastic process with constant birth- and death-rates λ and μ (with $\lambda > \mu$), the probability that a given ms will eventually leave j traces is

$$\frac{\mu(\lambda - \mu)^j}{\lambda^{j+1}}$$

Consider now the progeny, as $t \rightarrow \infty$, of a ms which is known to be the archetype. The probability that it will leave exactly two traces, when we know that it must (by definition¹) leave not less than two traces, is

$$\frac{\Pr(T=2)}{\Pr(T \geq 2)} = \frac{\mu(\lambda - \mu)^2 / \lambda^3}{(\lambda - \mu)^2 / \lambda^2} = \frac{\mu}{\lambda},$$

1. We need not be troubled by the exceptional case mentioned on p. A:53 n; for the probability that the archetype is itself extant will be limitingly small at the time discussed, namely $t \rightarrow \infty$.

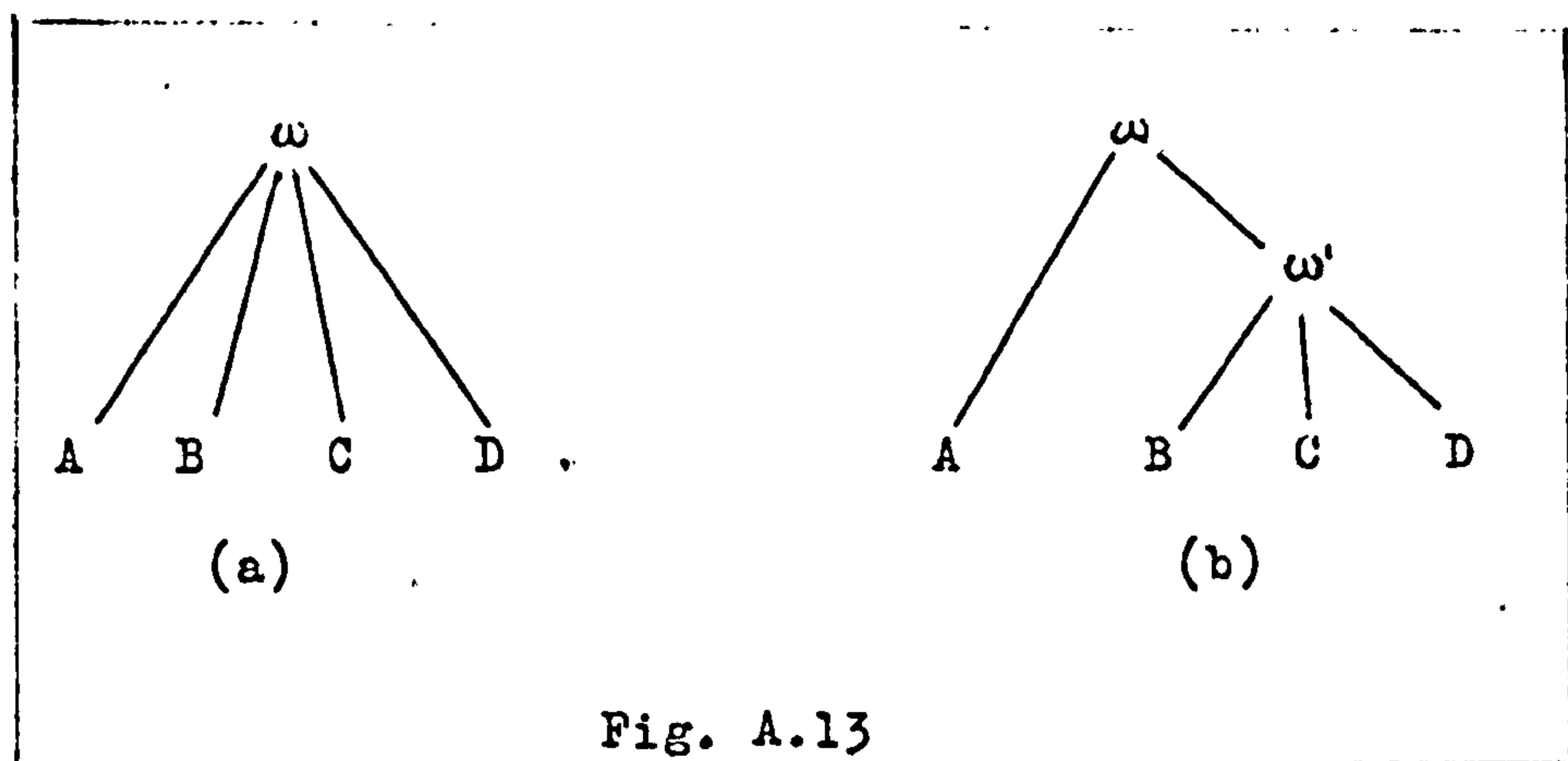
which we recognise once again as equal to the probability that a given ms should eventually leave no trace; and we recall that that probability is high. According to this much simplified model, then, a prevalence of two-branched stemmata is just what we should expect.

Admittedly, the foregoing analysis is impaired by the fact that it properly relates to the situation as $t \rightarrow \infty$, while the time interval with which we are in fact concerned is that which elapsed between the writing of the archetype and the present day. Even so, it is instructive, and suggests a more general argument. A ms produced in the period in which the archetype originated, is a priori far from certain of leaving even one trace; in order to 'qualify' as an archetype at all, a ms must necessarily leave at least two traces; we cannot therefore be surprised if the great majority of archetypes have not achieved more than the minimum requirement of two traces.

One further point may be appended. J. Irigoin has warned¹ of the possibility that a stemma which in fact has many branches may appear to be (and for practical purposes is perhaps better regarded as) two-branched. Manuscripts often 'evolved' during their life-time (e.g. by being corrected, or through physical mutilation). If the archetype has left three or more extant lines of descent (as in fig. A.13a), but was altered between the production of the oldest copy and that of the subsequent copies, then those later copies (and their progeny) will share a number

1. "Stemmas bifides et états des mss", in *Révue Philologique* (1954), pp. 211 ff.

of errors of which the first is free, and will appear to have a common source later than the archetype. We shall then postulate a two-branched stemma - as in fig. A.13b, where ω' purports to be an exclusive common ancestor of BCD, but is in fact ω in a second 'state'¹. Now our above result (p. A:60),



that the archetype was probably one of the longest-lived mss in the whole tradition, is of relevance here; for if such changes of 'state' did occur, the archetype is particularly likely to have undergone them. The likelihood is thereby enhanced that the factor which Irigoin pointed out did have some effect, and that some of the two-branched stemmata set up by editors are due thereto.

We may conclude, then, that a great majority of bifid stemmata is not so incongruous a result as Bédier imagined, and that it ought not in itself shake our faith in the validity of the stemmatic method.

1. R. Marichal (op. cit. in Thes., p. 11:1n), p. 1284, believes this argument to be applicable to classical Greek and Latin texts, but hardly to texts in mediaeval Latin or French.

§12. The sampling of a rich tradition

The editor of a text which is preserved in a great number (often to be measured in hundreds) of mss, finds himself in a dilemma. At one extreme, he might decide to collate them all. In that case, he will soon find that the collation of each new ms adds less and less - in terms of valuable readings of which he did not know before - to the sum total of his knowledge; and eventually (say after he has collated 30 mss) he will reach a point at which he very much doubts whether the ground to be gained by soldiering on to the end will justify the seemingly endless effort, and the delay in the appearance of his edition. If, on the other hand, he studies no more than a fraction of the textual evidence, he can never be absolutely certain that he has not passed over important material which he could have utilised; the danger will be all the more serious if there are many instances of isolated preservation of ancient readings. We may therefore wonder whether it is possible to recommend a strategy whereby we may sample a tradition too rich to be collated in its entirety, in such a way as to minimise the risk of neglecting important information.

The dangers of various sampling methods to which scholars have resorted, are pointed out by Pasquali. It has been a common practice to concentrate attention on the oldest mss at the expense of the more recent ones, sometimes on the grounds that the latter are either descriptive of the

former or removed from the original by so many intermediate copies as to be too corrupt for use. But our work in §§3-9 shows those grounds to be inadequate, and many examples are cited by R-W (pp. 146 f) of a late ms that contains ancient readings not to be found in any of its extant predecessors, which may be as much as three centuries older. Equally dangerous is the device of basing one's selection on one or more short test passages; many a ms changes its character in the course of a work¹, and a small proportion of the text could easily give a very inaccurate impression of its value².

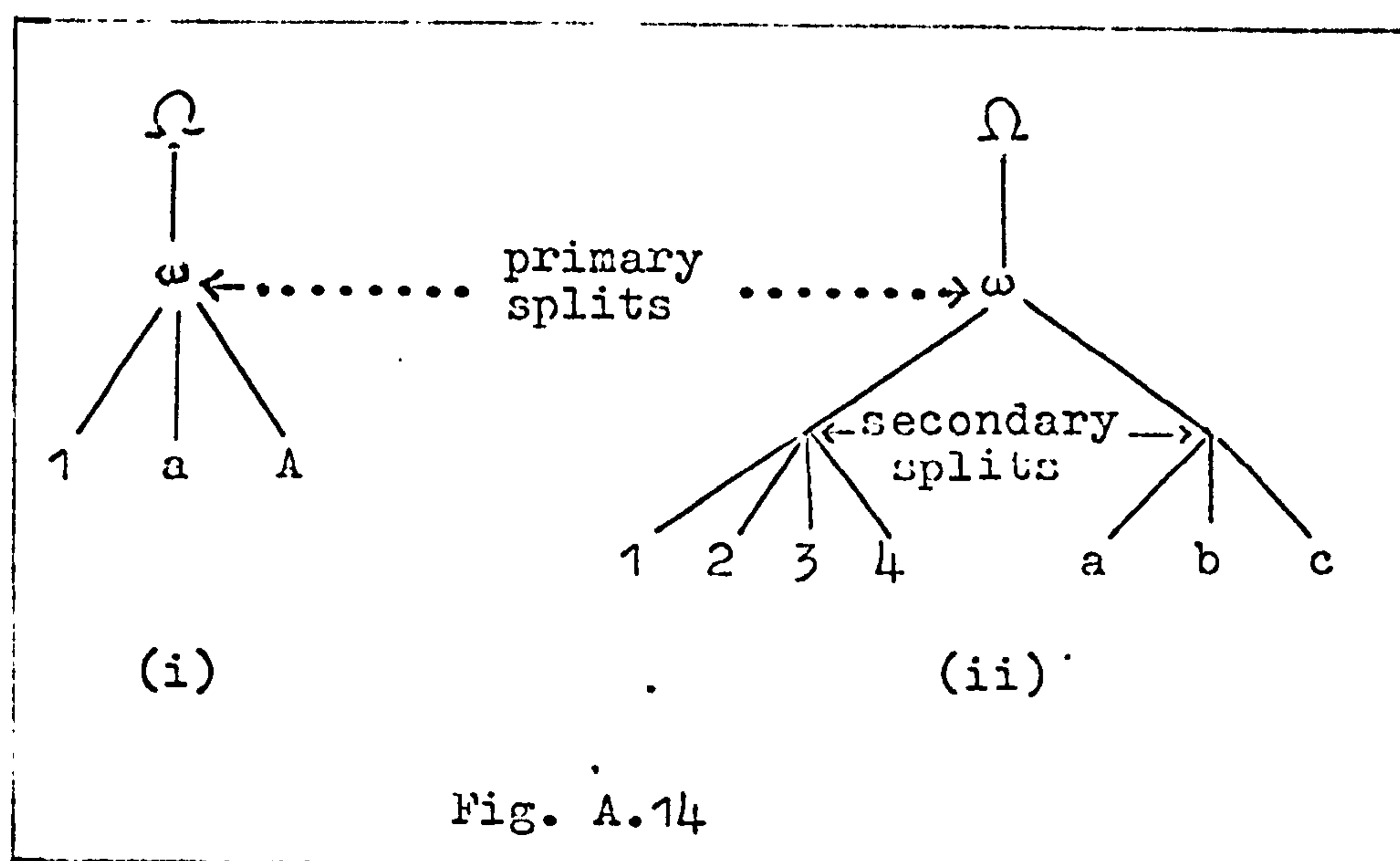
There is always some risk in working with a sample of mss; but we may be able to shed some light on the problem if we can devise a sampling procedure which would have produced in every case a satisfactory sample if it had been applied to our fifteen artificial traditions. We begin by specifying the following minimum requirements for the sample to meet:

1) In those experiments in which the primary split is into three or more branches, at least three of these branches should be represented.

2) In those experiments in which the primary split is into only two branches, each should be represented, and if there are secondary splits, at least two branches of

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1. For statistical evidence on this point, see pp. 3:18 ff.
 2. See further Pasquali, pp. 36 ff, and also Dawe's criticism of the results reached by A. Turyn on the basis of samples (Thes., pp. 6:36 ff).

each should have representation within the sample.



The consideration on which this definition is based is that within a tradition to which the Maasian system can be applied, a sample that meets these requirements will be just as useful when one fixes the text of the archetype (except in certain atypical passages) as a full collation of all the available evidence; while a sample that fails to meet them may frequently leave the text in doubt which could be dispelled by further collation.

Consider, for example, a primary split into four branches. If our sample contains representatives of three of them, we shall generally be able to fix the text of the archetype by the agreement of any two branches; only in the atypical case of all three offering different readings, will there be any need to consult the fourth branch. On the other hand, if no more than two branches are represented in our sample, we shall have to hesitate whenever the two branches disagree, and a sample which included a representative of a third branch would almost always have resolved the difficulty.

We now examine each of the fifteen traditions¹, and determine which primary branch of the tradition - and, for those experiments in which there are only two primary branches, which secondary branch - each constituent ms belongs to. The result is shown in fig. A.15, in which each ms is represented by an appropriate symbol, the following code being used. Every primary branch is assigned a particular class of symbol. In no case were there more than three such branches; three symbol classes were therefore used, namely numeral (123...), small letter (abc...), capital letter (ABC...). These symbol classes are employed in chronological sequence; that is, the progeny of the oldest hyparchetype (i.e. direct copy of the archetype) will be denoted by numerals, that of the second oldest by small letters, and so on. When we do no more than specify the primary branch to which each ms belongs, the first of the available symbols in the appropriate class is used (1, a, A). In other cases, secondary branches within one and the same primary branch are distinguished from one another through the use of different symbols (again in chronological sequence) within the class assigned to the primary branch. Reference to fig. A.14 should dispel obscurity.

Fig. A. 15, then, exhibits the extant mss of thirteen artificial traditions in chronological order of 'birth', each ms being represented by the appropriate symbol. Inspection

1. apart from no. 1 (in which one sole ms survives) and no. 7 (two mss, one descended from the other).

of the Figure shows that, in most of our experiments, a sample consisting of the six oldest mss will fulfil our requirements, but not in all (see nos. 2, 9, 10, 11); representatives of a new secondary or even primary branch sometimes appear among the more recent mss. An instructive example is furnished by the ninth experiment. Here, the archetype was born in the year 25, and three direct copies thereof, dated 45, 249 and 320, have left surviving descendants; thus the stemma is three-branched. The earliest extant ms in the tradition, born in 689, derives from the second of the three 'brothers'; the second oldest ms was produced in 800, and is a descendant of the youngest hyparchetype; and not before the seventeenth ms, of the year 924, do we find our first representative of the family proceeding from the hyparchetype written in 45. If the archetype had been damaged after the year 45, we could be pleasantly surprised to find, among a few comparatively late mss, ancient material lacking in all the earlier authorities.

Perusal of fig. A.15 suggests that we would be likely to obtain a sample fulfilling our requirements if we were to select (i) the six oldest mss, and (ii) a further five drawn at random from the latest third of the tradition. This tentative result is based, of course, on our artificial traditions alone; but it deserves to be tried out on the traditions of real texts. In such a case it would be most desirable to add, with Dawe (*op. cit.*, p.16), 'a few (say five or less) mss as a control, chosen "on no more scientific principles than suspicion, caprice, and the

attractiveness of the cities in which they were located". These additional mss will help us to judge how much valuable evidence we would be missing if we did not go any further with the task of collation. In any event, we shall have at this point a collection of some fifteen mss, which is certainly small enough to be analysed without serious difficulty through various techniques described in Section B (e.g. multidimensional scaling, seriation); this done, the investigator will have some firm ground under his feet as he ponders his next move.

* * * * *

The work of Section A, then, has set out to elucidate certain general problems that arise in textual criticism. The student of any particular tradition will, it is hoped, benefit from having these results at his elbow; but they will not suffice in themselves to guide him through the network of relationships between the various mss that make up the textual tradition which he is investigating. It is to this latter problem that Section B, to which we now proceed, is devoted.

SECTION B

The classification of manuscripts,

with particular application

to the Peshitta Psalter

1. The Theory of the Stemmatic Method

The original aim of this thesis was to contribute to the textual criticism of the Old Testament Peshitta. It seemed to me that far more progress had been made in collecting textual data than in interpreting it, by which term is meant the development of methods for reconstructing the history of the text, for choosing between alternative readings when the mss disagree, and for reaching as accurate a Peshitta text as our rich material allows. It is therefore with methods for interpreting the textual evidence that Section B of the thesis is principally concerned.

The task of preparing a critical edition of the Peshitta O.T. has been undertaken by the Peshitta Institute in Leyden University¹. The editors warn us that "the time has not yet come to produce a reconstruction of the 'original' Peshitta Old Testament."² Indeed, the last word on the subject will not have been said even when the whole tradition, direct and indirect, has been subjected to a thorough analysis. Accordingly, the edition is to present in the text the readings of one particular ms³, which is "printed unchanged, except for the correction of

1. For details on the impressive achievements of this project so far, see p. 10:38.

2. "The Old Testament in Syriac...Sample edition", Leyden 1966; General Preface, p. [VI].

3. Usually Codex Ambrosianus.

obvious clerical errors that do not make sense" (p. [VIII]). The readings of the other mss, when they diverge substantially from the basic text, are recorded in a critical apparatus. Thus no attempt is made to determine which reading is 'original' when the mss diverge. Of course, this situation is not wholly satisfactory. Thus M. H. Goshen-Gottstein¹ comments (p. 63n.) that "...merely printing a collection of variants means leaving half the work undone. The sometimes rather arbitrary decisions of nineteenth-century scholars in preparing eclectic text editions have, to be sure, brought about a reaction and taught modern editors to be more cautious. But if this cautiousness leads to hyper-objectivity, which causes the editor to fall into the other extreme and to abstain altogether from even indicating his preference - so as not to 'prejudice' his readers - he will hardly have discharged his duties as editor of a critical edition." Most future readers of the proposed edition were in danger of being left "without any indication as to how to get out of the bewildering labyrinth of variants." No doubt the Leyden editors have themselves been aware from the start that it would have been more satisfactory had it been possible to choose confidently between the variants; and their decision to give up this aim for the moment surely is not based on doctrinaire theorising, but reflects the difficulty of the task. Thus I felt it was not too early to make a start

1. "Prolegomena to a critical edition of the Peshitta", in *Scripta Hierosolymitana* (1961), pp. 26-67.

now on methods for sifting the textual data, though we must acknowledge that we cannot hope for certainty. We may also hope that such investigations will be of use to the Leyden project itself, not least in suggesting which are the most promising directions in which to continue the search for textual material.

There are two ends from which our main problem, viz discrimination between rival readings, can be approached. On the one hand there are the intrinsic criteria; we may consider the nature of the readings, and inquire which of them accords best with what is known from elsewhere about the normal usage of the language and the author, which of them best explains how the alternative readings arose¹, and so on. On the other, there is the evidence which we may call distributional²; we may choose between variant readings by considering in which mss each one is carried (in other words, the distribution of that reading among the mss), and by utilising our knowledge of the history of

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1. One critic who invoked this principle many times was Edward Moore, in his "Contributions to the textual criticism of the Divina Commedia", Cambridge 1889. He states (p. xxxv): "Perhaps the soundest and most general of all critical principles is this: If the prior existence of one out of two or more various readings would naturally account for the genesis of the others, while the supposition of their prior existence would not account for it, that reading is most probably the true and original one." The rule of "difficilior lectio potior" is, when properly applied, a particular instance of this general principle.
 2. I have borrowed the term from A.A.Hill, "Some postulates for distributional study of texts", in Studies in Bibliography (1950), pp. 63-95. His definition is: "The arrangement of differences in the surviving texts can be studied, without attention to the nature of the differences. This constitutes distributional study." However, my work is on a rather different footing from Hill's, as will become apparent.

the text we may decide how likely it is that a reading attested in a given set of mss is original. I believe that neither of the two approaches can safely be used to the exclusion of the other. Conclusions reached on intrinsic grounds will often be open to question; scholars will not always agree which of the alternative readings is "best", however we define that term. However it has been shewn¹ that, unless we are to commit ourselves to unrealistic assumptions, we can never set up distributional rules for choosing with certainty between variants; we may speak only of relative degrees of likelihood, and the character of the readings themselves must always be borne in mind². Thus the first approach without the second is too subjective; the second without the first is too rigid. The two are complementary, and confidence in our results is at its greatest when both methods point in the same direction.

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1. See discussion in Chapter 2.
 2. The remarks of A. E. Housman, "The application of thought to textual criticism", in Proceedings of the Classical Association (vol. xviii, 1921), deserve to be quoted at some length: "...Textual criticism is not a branch of mathematics, nor indeed an exact science at all. It deals with a matter not rigid and constant, like lines and numbers, but fluid and variable; namely the frailties and aberrations of the human mind, and of its insubordinate servants, the human fingers. It therefore is not susceptible of hard-and-fast rules. It would be much easier if it were; and that is why people try to pretend that it is, or at least behave as if they thought so."

As far as the Peshitta O.T. is concerned, a great deal has already been accomplished in the investigation of intrinsic criteria. Individual studies, in varying depth, have appeared of the translation technique employed in most of the O.T. books¹, and these tell us a great deal about what a given translator was or was not capable of writing. I have done some work on these lines with reference to Jeremiah 46-51 (see Section C); but most of my energy has been devoted to the distributional approach, which has been applied to the Psalter (see Chapters 7-10 of the thesis). This has come about mainly because of my interest in mathematics; I am by no means the first to have appreciated that the study of ms inter-relations and the establishment of distributional "rules" may be viewed as a statistical problem. It should be made clear immediately that although I use the convenient term "rule", I do not have in mind any "règle de fer"² but a procedure to decide which reading has the advantage on distributional grounds. That distributional evidence will have to be considered in conjunction with the intrinsic facts before a decision can properly be made.

1. For a summary and bibliography, see L. Haefeli, "Die Peschitta des alten Testaments", Münster 1927 [= Alttestamentliche Abhandlungen. XI. 1]. The Peshitta Institute keeps a virtually complete and up-to-date bibliography of all literature relevant to the O.T. Peshitta.
2. This was the aim of Dom H. Quentin in his "Mémoire sur l'établissement du texte de la Vulgate" (Rome and Paris, 1922), p. ix: "...je propose une méthode qui part de collations minutieux pour aboutir à une règle de fer."

In my search for a distributional policy, I first turned to the most widespread method, viz the reconstruction of a stemma or family-tree within which the extant mss can be located. The classic exposition of stemmatics, as the study of stemmata may be termed, is that of P. Maas¹. Its basic principle is that agreements in error among a given set of mss indicate that those mss have a common origin. It is easy to derive the stemma from the textual evidence and to use it in almost every case to choose between rival readings; Maas explains how to construct the stemma and how to apply it (in cases where it yields an unequivocal answer), within the space of five pages.

Yet the stemmatic method has fallen into discredit in many quarters; indeed, it could hardly be said ever to have come into its own in the study of O.T. textual problems at all. Typical is the complaint of Dom Bonifatius Fischer,² who states (p. 305) that the result of his long experience of textual study "is, to be honest, a profound scepticism and a certain doubt, not only about the smaller papers in which far too extensive conclusions are drawn from a few variants, but more generally about the various methods at present used in textual criticism - indeed, my doubt embraces the theoretical basis of textual criticism as

1. "Textkritik", Leipzig 1960 (4th ed.)

2. "The use of computers in New Testament studies, with special reference to textual criticism", JTS (1970) pp. 297-308.

universally accepted and taught nowadays. The use of common errors to construct a stemma codicum and the construction of a text on that basis rests on a false theory and comes to grief in practice when the tradition is more complex or merely more extensive."

Since this method offers such promising returns on the one hand and has been so much criticised on the other, I thought it best to embark on an analysis of what has to be assumed in order for the stemma to function. What is offered in the following pages owes much to the work of Sir Walter W. Greg¹ and Dom J. Froger², as well as that of Maas; it can nevertheless claim a certain independence.

A brief introduction to the works of Greg and Froger is appropriate here. Greg aimed to set up a formally logical background to establish rigorous procedures for deriving the stemma from the textual data. Though aware that the use of mathematical symbolism might have rendered his treatment more succinct (p. v), he preferred to write in terms which were essentially familiar to textual critics. Questions which could have been simplified had they been formulated in terms of the mathematical symbolism of set theory etc. are discussed throughout in concrete terms like mss, readings and so on³. It would be fair to say that to

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1. "The calculus of variants: an essay on textual criticism", Oxford 1927.
 2. "La critique des textes et son automatisaion", Paris 1968.
 3. Indeed the only stemmata which are actually drawn are in an appendix (pp. 60f.).

read Greg's book takes some considerable mental effort (although the effort is well rewarded), and this may be why its impact on the academic world has been somewhat limited. Froger's book is, I feel, far more useful - at least initially - than Greg's, although he is not concerned with rigorous proof. He is not afraid to introduce set theory and elementary topology, which very much clarify the exposition, although I wonder whether all the mathematical material in his book (especially pp. 139-216) is immediately relevant to his theme¹. Thus room was left for a treatment which isolated the assumptions on which the procedure rested, and shewed rigorously how that procedure could be justified.

In the present study, we first set up a series of assumptions² and shew how each is involved in working out the theory both of the reconstruction and of the application of the stemma. The reader need not be alarmed to find these assumptions unrealistic; what matters at this stage is the logic whereby the theory of stemmatics follows from them. The assumptions themselves will be examined in Chap. 2. It should be pointed out that this treatment does not claim to be the only possible way of developing a formally logical view of stemmatics.

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1. It is remarkable that although Greg, Maas and Froger had the same end in view, none of them mentions even in a bibliography the work of either of the other two.
 2. On the assumptions set up by Greg, Maas and Froger, see pp. 1:40 ff.

We first need a term to denote the stretch of text which is being contemplated. This I would call the "domain". A domain may consist of the whole text on which the editor has undertaken to work, or a part of it, or two or more discrete portions. We could for example speak of a domain which consists of fifty-odd passages excerpted from a whole treatise.

Suppose that we wish to draw a stemma, to be valid over a certain domain, which we term the "validity domain". In order to draw a tree diagram at all, we need two assumptions:

- I. The original is uniquely defined throughout the validity domain.

Thus at every point there is only one original reading; we exclude from the domain passages in which the author himself left alternative readings between which he never made the final choice¹. In the context of O.T. versions in particular, (I) cannot be reconciled with the 'Kahle view', that in many passages where the mss diverge this is because they preserve two or more different attempts at translating the Hebrew original into the language in question².

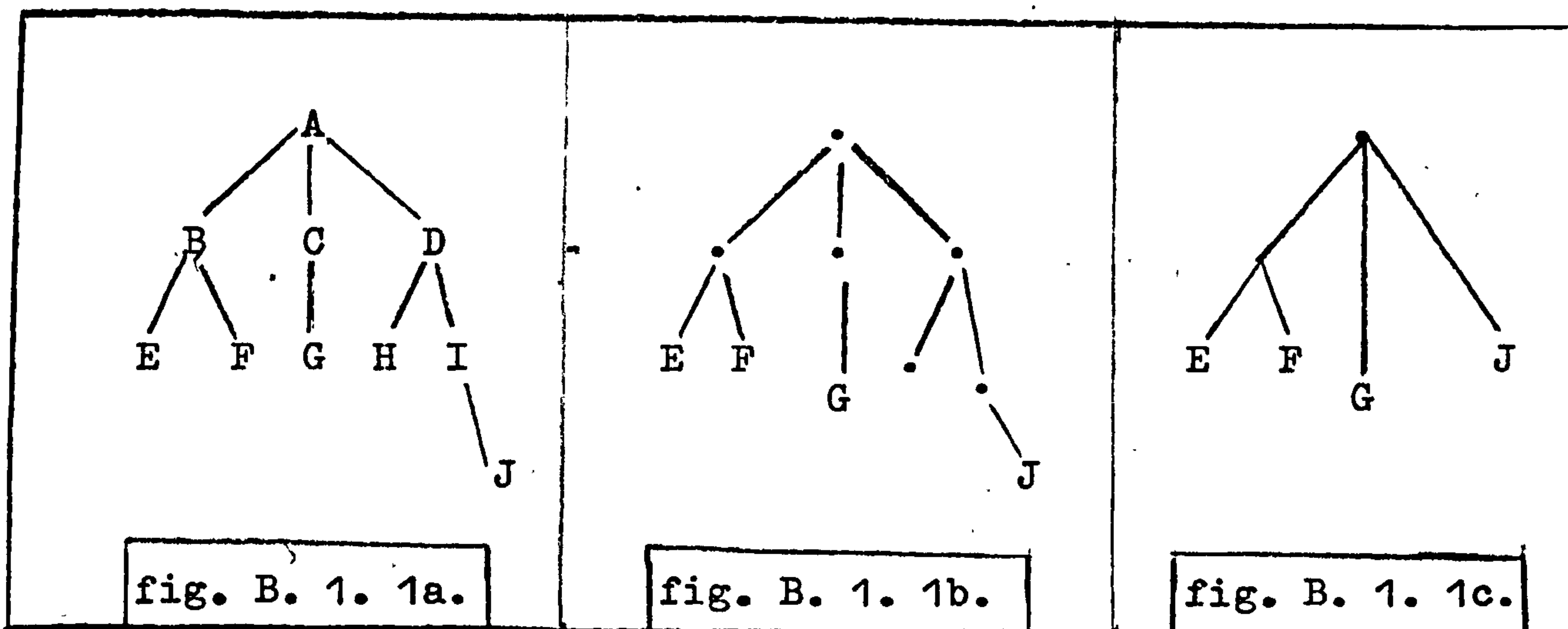
1. On this possibility, see G. Pasquali, "Storia della tradizione e critica del testo", Florence 1952, (2nd ed.), Chap. VII.
2. The 'Kahle view' in relation to the Peshitta is discussed on pp. 8:1 ff.

II. Every ms, apart from the original, is copied from only one ms (its 'immediate ancestor' or 'exemplar').

We can accommodate most cases of conflation, in which a scribe had two or more exemplars before him, by defining the immediate ancestor of a ms M as that ms which was 'alive' when M was produced, and which diverges textually the least from M (cf. p. A:5).

We may then draw a tree of all the mss - both extant and lost - collated over the domain, joining each ms to its exemplar, as on p. A:30 . This is not on the same footing as most stemmata, because in most traditions there are many lost mss, and the stemma gives only those lines of the full tree-diagram which are needed to explain the relationships between the survivors.

How then do we proceed from the full tree-diagram to the stemma? Let us consider a ms tradition of altogether 10 mss ABC...J, whose genealogy is indicated in fig. B.1.1a. If of these only EFGJ survive, we could trace their history (if we knew it) in the form of fig. B.1.1b, in which a dot represents a lost ms. But we still do not have a stemma. In order to obtain one, we must (a) remove branches which do not lead down to any extant ms, and (b) telescope a series of lines separated by dots into a single line. This yields fig. B. 1. 1c:



The steps whereby we derived the third figure from the second seem innocent enough; but are they in fact justified? They imply (a) that a ms which has itself been lost and which leaves no extant descendant may be ignored, and (b) that a series of transcriptions is indistinguishable from a single one (i.e. that we do no harm if we suppose G and J to be direct copies of the original, although they are not).

In order to justify these steps, we need a further assumption:

III. No ms, extant or lost, took over the readings of any ms other than its exemplar, throughout the domain.

Hence, the readings of a ms which is lost and has no extant descendant cannot have been transferred into any line of descent surviving today, and so we can act as if that ms had never existed - (a). Furthermore, let us consider the effect of a single copying. This will be limited to (i) the

introduction of errors where the exemplar has the correct reading, (ii) the removal by successful conjecture of errors in the exemplar, and (iii) the replacement of an incorrect reading in the exemplar by another incorrect reading. Any agreement between a reading produced in the copy and the reading of any other ms (except for the exemplar) will be coincidental. Now let us consider the effect of a series of copyings. This too will consist of a combination of (i), (ii) and (iii), which could theoretically have arisen in a single copying - (b).

If, however, certain lost mss which left no direct descendants nevertheless transmitted some of their readings to other lines of descent and possibly to extant mss, then we evidently cannot ignore those mss with impunity; and any stemma which neglects them will be misleading. Only over a domain within which every ms is derived strictly from one exemplar are we entitled to think in terms of a stemma, as opposed to a full tree-diagram.

The question of which mss have survived - or which have been singled out for study - results in mss being represented on the stemma in different forms. Some are represented by points; these either are extant (EFGJ) or have left two or more lines of descent (AB). Others are to be found along lines (or arcs, to use the terminology of graph theory); these are lost mss which have extant descendants but have left only one line of descent (CDI). Finally there are those which, having failed to survive or leave any descendants, have no representation at all (H). The three types may conveniently be termed point mss, arc mss and traceless mss. Traceless mss can be ignored - (a); the effect of the transcription represented by an arc ms can

be referred to the point ms at the lower end of the arc - (b); and thus our whole analysis can be executed in terms of the point mss alone. The topmost point ms, which is the latest common ancestor of all the extant mss, is termed the "archetype".

All this makes for enormous simplification of the textual history, as a few figures will illustrate. If the total number of extant mss is E , then the number of point mss which are not themselves extant cannot¹ exceed $(E-1)$. Thus, no matter how many mss may at some time have carried a given text, it is possible for the stemmatist who has 7 mss at his disposal (whether this is because they are the only survivors or because it is inconvenient for him to collate any others) to study their history by considering those 7 mss plus not more than 6 hypothetical ancestors - 13 mss in all, although the complete tradition may have contained several hundreds. Again, on p. A:30, a tradition of 77 mss altogether left in the end 15 survivors, which could be arranged in a stemma, together with only 4 lost ancestors; what survived of the whole tradition could be viewed in terms of 19 mss, while the fact that the other 57 had ever existed at all could be ignored. But all this simplification rests, I repeat, on assumption (III).

The following is one way of interpreting the stemma. Suppose that we formally define the set of ancestors of a ms M to consist of (i) the original, (ii) any later ms from

1. The number $(E-1)$ will be reached in the most favourable case, viz a wholly binary tree.

which M is derived, down to the exemplar of M , and
 (iii) M itself. Thus a k th-generation ms (p. A:46) will have $(k+1)$ ancestors¹. Similarly, we may define the set of descendants to consist of M itself and any ms derived from M . We now find that every point ms is an exclusive common ancestor (or, for short, xca) of all its extant descendants². Thus, in fig. B. 1. 5a, α is an xca of ABC, D is an xca of DEF, A is an xca of A, and so on. Any set of mss has a common ancestor, but it is not necessarily exclusive. Suppose that we now enumerate, for each point ms in turn, all the extant descendants of that point ms. Then we shall obtain a list \mathcal{L} of ms sets, each of which possesses an xca. Conversely, no set of mss which has an xca will be absent from that list. For, let ξ be the xca of any ms set. Then ξ is either a point ms or an arc ms. In the former case, the set of its extant descendants will have been included in \mathcal{L} . In the latter, any extant ms derived from ξ will also be a descendant of that point ms at the lower end of the arc on which ξ lies, and thus the set of extant descendants of ξ will again have been included in \mathcal{L} . Thus the stemma may be regarded as a complete statement of which ms sets possess an xca. For the stemma of fig. B. 1. 5a, the list \mathcal{L} will read: A, ABC, B, BC, C, DEF, E, EF, F.

In general, of course, the editor does not know the stemma in advance; he knows only the readings of the mss.

1. Note the contrast with p. A:46, where the set of ancestors of a ms does not include that ms itself.
2. The concept of an xca plays an important role in Greg's book (p. 4 and elsewhere).

The problem to which we now turn is, how we may derive the stemma from the textual evidence. We do not know whether (III) is fulfilled in the relevant domain, and therefore whether we are justified in trying to draw up a stemma at all; but we provisionally assume (III), and see whether it is a workable hypothesis.

We may occasionally be helped by external evidence, e.g. a colophon which states that a certain ms was copied from another which can be identified today; but in the main, we must look primarily to the internal evidence of the text itself, and the way in which the mss group themselves when they diverge.

When the mss offer two or more variant readings, we may view this either objectively (stating e.g. that AB have a different reading from CD) or subjectively (stating e.g. that AB have the better reading against CD). The subjective statement goes further than the objective, and implies that the intrinsic criteria have sufficed to tell us which reading is authentic. Clearly it would be desirable to use throughout objective statements only, were it not for the following result, which we term the "theorem of ambiguity", viz: The stemma cannot be uniquely identified through the objective evidence alone. This theorem will be proved below.

We first observe that our assumptions so far do not rule out the possibility that two mss may come to have the same reading by coincidence; therefore any ms grouping

could occur, theoretically, whatever the textual history. In the situation of fig. B. 1. 1, for example, we might find that EJ preserve the correct reading against FG, which have independently committed the same error. In order to get any further, we must extract from the validity domain a set of passages on which our construction of the stemma will be based, and in which additional assumptions are made whereby the scope of coincidence is circumscribed. We further stipulate that this "construction domain" should not contain any passage in which the readings of all the mss are not available.

Two assumptions regarding coincidence are required, namely:

- IV. If two different copyists each deviate from their exemplars at the same point in the text, they will not produce the same reading.
- V. No copyist successfully corrects an error in his exemplar.

From these two assumptions, we may deduce the following corollaries, valid over the construction domain:

Suppose that a certain set of mss have the same incorrect reading. Then this error arose in a single ms, which is a common ancestor of all the mss bearing that incorrect reading. No ms which has the correct reading can be descended from that common ancestor (Lemma 1).

Thus we have eliminated the possibility of coincidental agreement either in erroneous (IV) or in correct (V) readings. Before working out a procedure for recovering the stemma from the evidence of the construction domain, we prove the theorem above.

Let us define a "split" as a division of our extant mss into two or more exhaustive and mutually exclusive classes. For example, three extant mss ABC may undergo four possible splits (A:BC, B:AC, C:AB, A:B:C). A split into n classes is termed an n -way split. The 'objective' evidence will consist of a complete list of the splits occurring in the construction domain.

In order to prove the theorem of ambiguity, we require the following lemma:

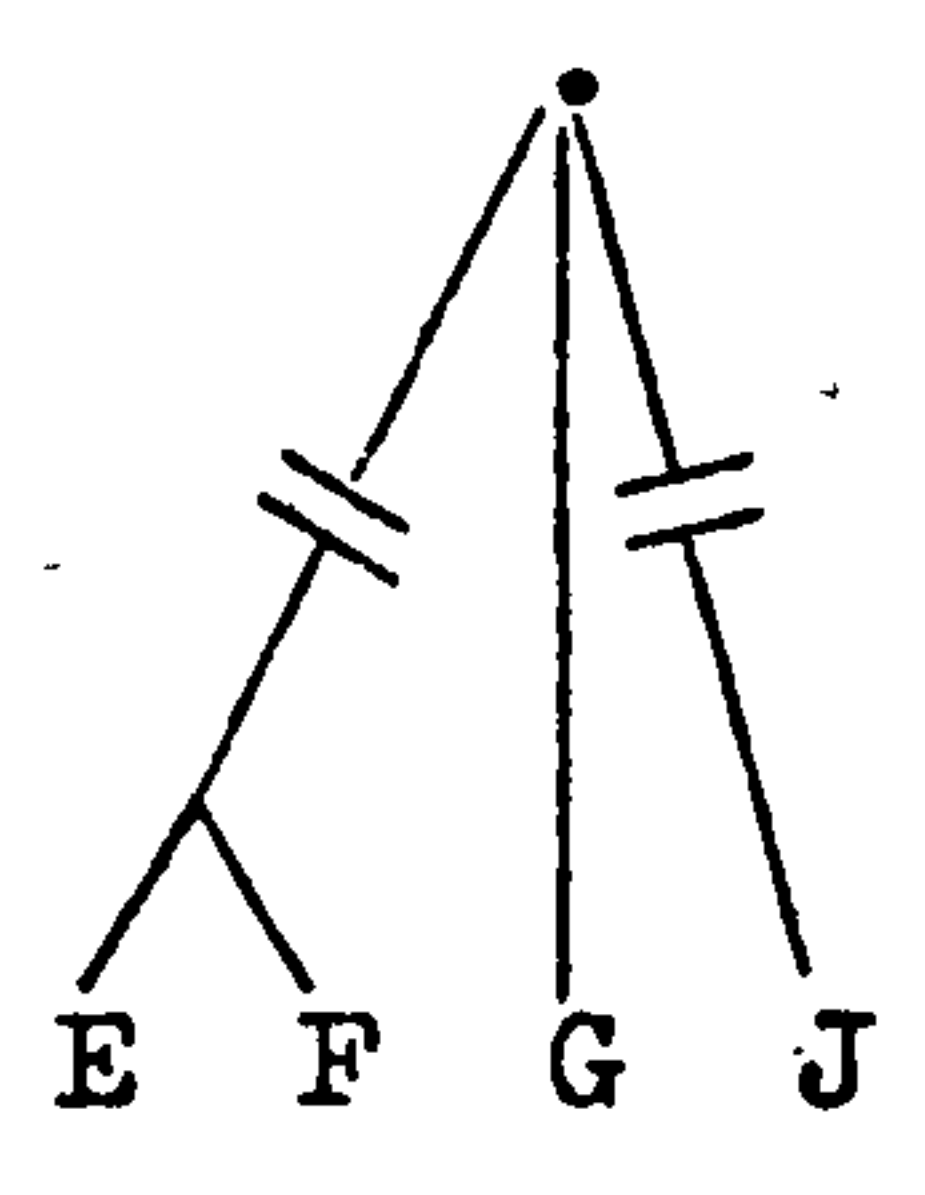
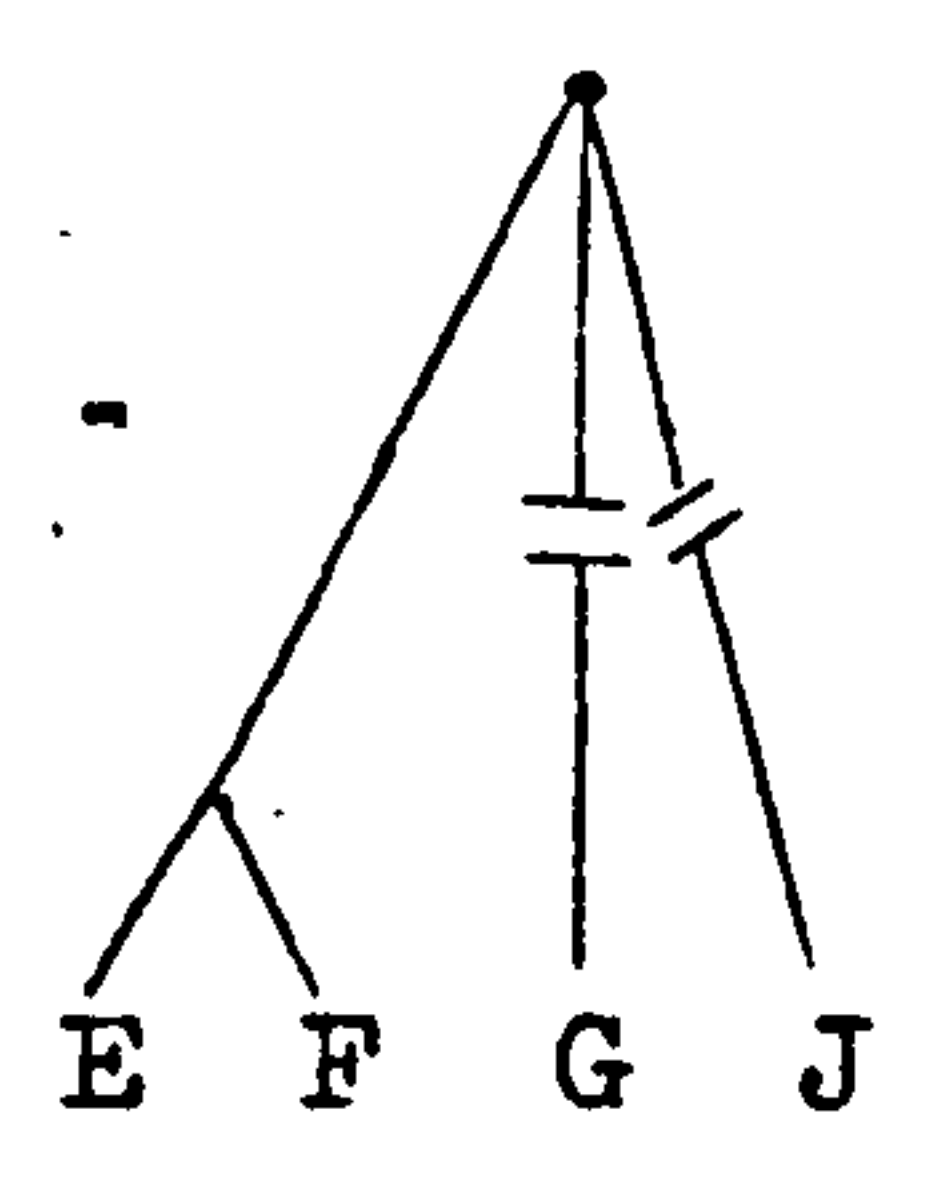
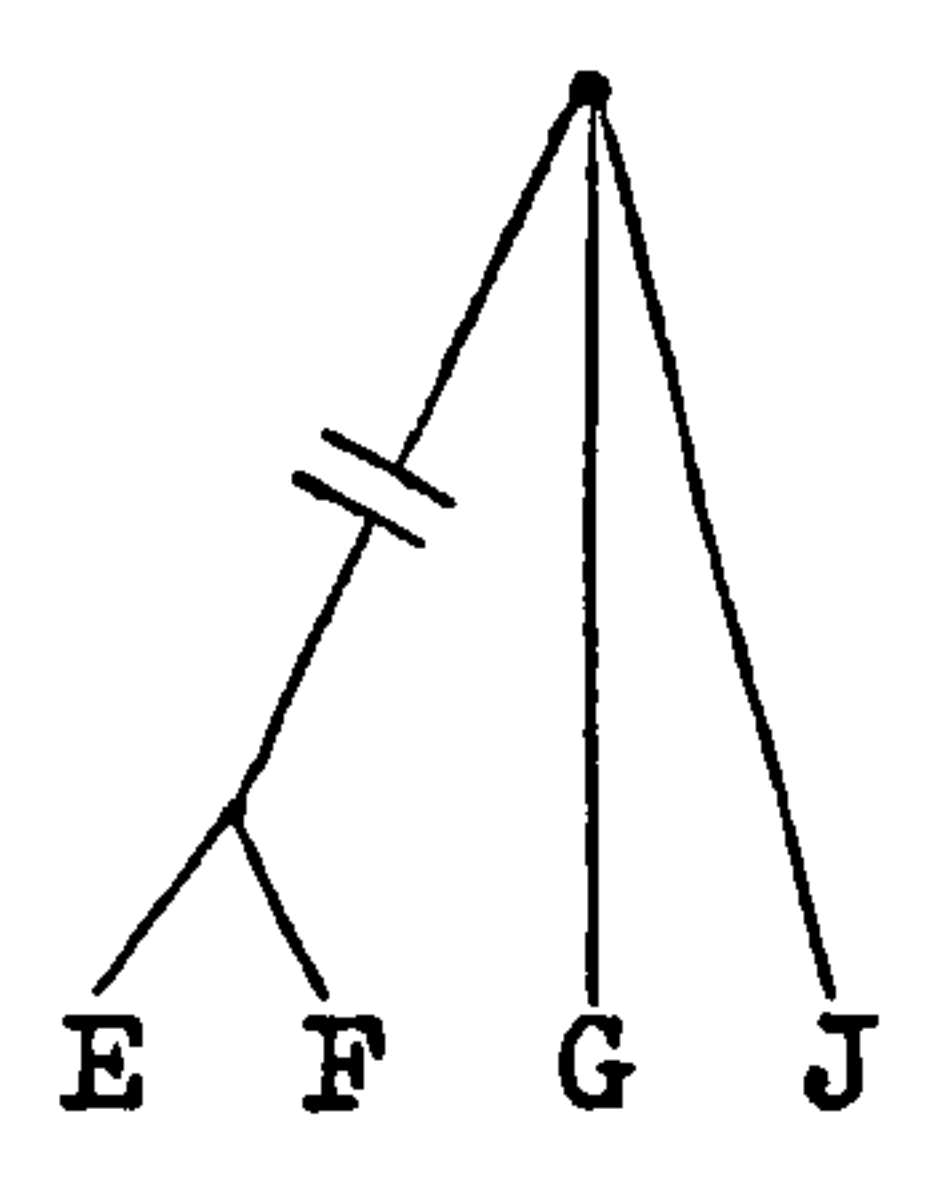
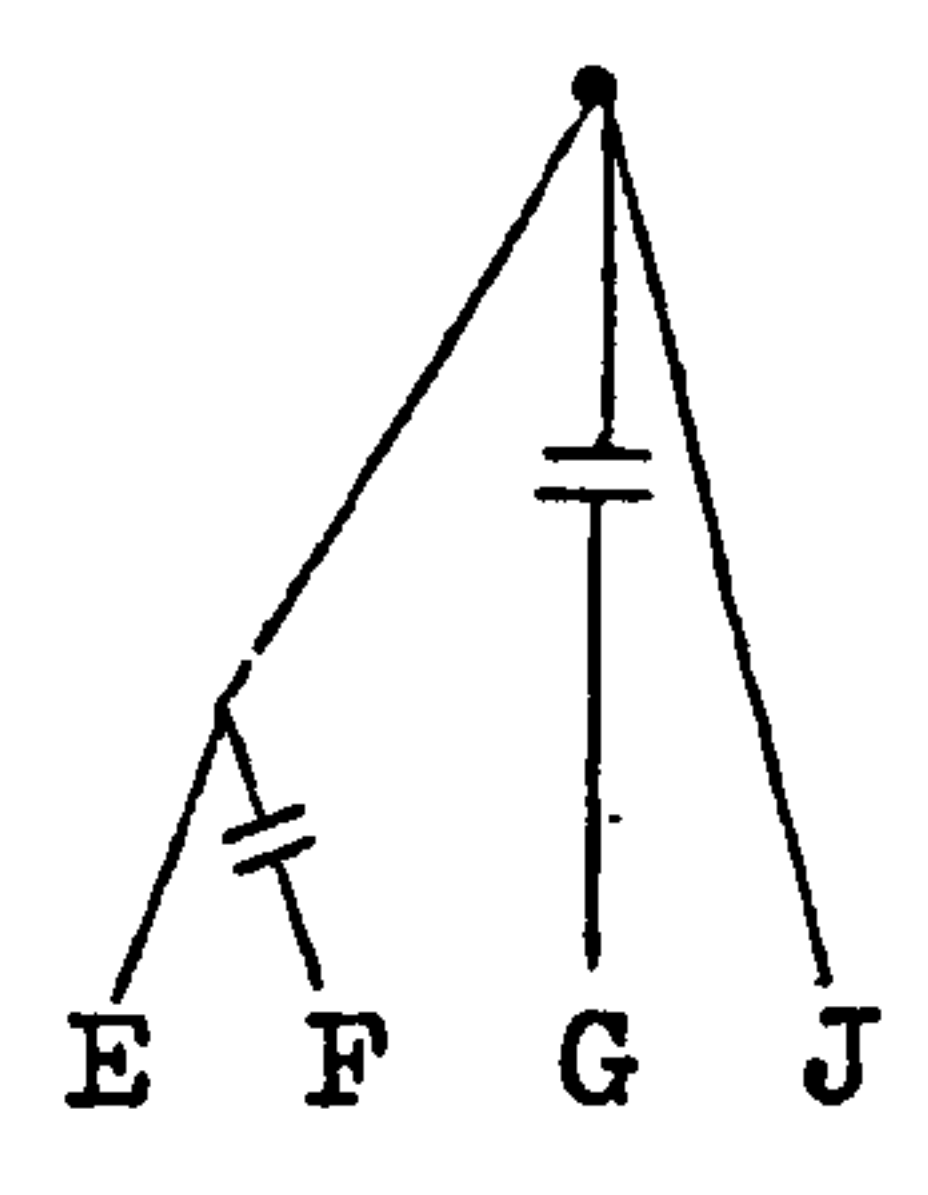
Lemma 2. Let S be any stemma which sketches the history of a textual tradition. If a k -way split among the surviving mss occurs within a domain where assumptions I-V hold, then it will be possible to cut $(k-1)$ arcs of S , and thus divide S into k sub-trees, of which each will be characterised by just one of the k variants, i.e. all extant mss lying in that sub-tree will bear that particular variant reading.

Proof. There are k alternative readings. Only one is original (I); the other $(k-1)$ are incorrect. Each of these $(k-1)$ errors goes back to one ms, which is a common ancestor of all ms bearing that error (Lemma 1). Each of these $(k-1)$ ancestors has left one or more descendants, and is therefore either a point ms or an arc ms; consider the arc along which it lies, in the second case, or the arc immediately above it, in the first. We have thus specified $(k-1)$ arcs, each associated with one of the $(k-1)$ errors. If we cut any one of these arcs, we divide the stemma into

two sub-trees. That which lies below the cut will consist of the descendants of the ms responsible for that error; and that error will be carried by all the mss in that sub-tree, unless one of the descendants deviates from its exemplar at the same point (V). In the remaining sub-tree, the error will not be found at all (IV).

Suppose that we now cut each of the $(k-1)$ arcs. Then we shall obtain k sub-trees. All mss in the sub-tree which contains the archetype will have the correct reading; and the other $(k-1)$ sub-trees will each be characterised by one of the $(k-1)$ incorrect readings. Thus Lemma 2 is proved.

An example will illustrate both the lemma and its proof. Consider the stemma of fig. B. 1. 1c. We might find, in the text, that our four mss EFGJ split in various ways, e.g. EF:G:J (through deviations in B and D - fig. B. 1. 2a - or in G and J - fig. B. 1. 2b), or EF:GJ (through deviation in B - fig. B. 1. 2c) or EJ:F:G (through deviations in F and G - fig. B. 1. 2d). In every case it is possible to cut the arcs of the stemma accordingly. However, a split EG:FJ could not be generated by arc cuttings; and this reflects the fact that such a split could not occur in a domain where assumptions I-V held good.

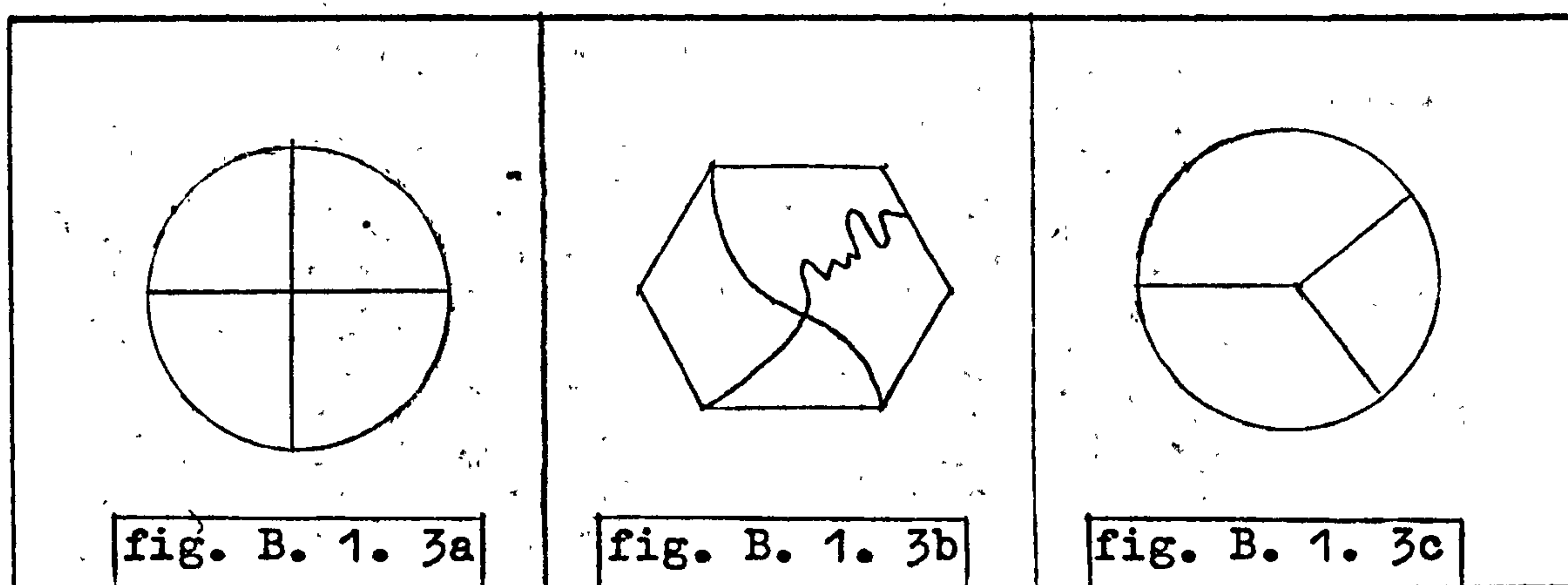
			
fig. B. 1. 2a	fig. B. 1. 2b	fig. B. 1. 2c	fig. B. 1. 2d

A third lemma is the converse of Lemma 2:

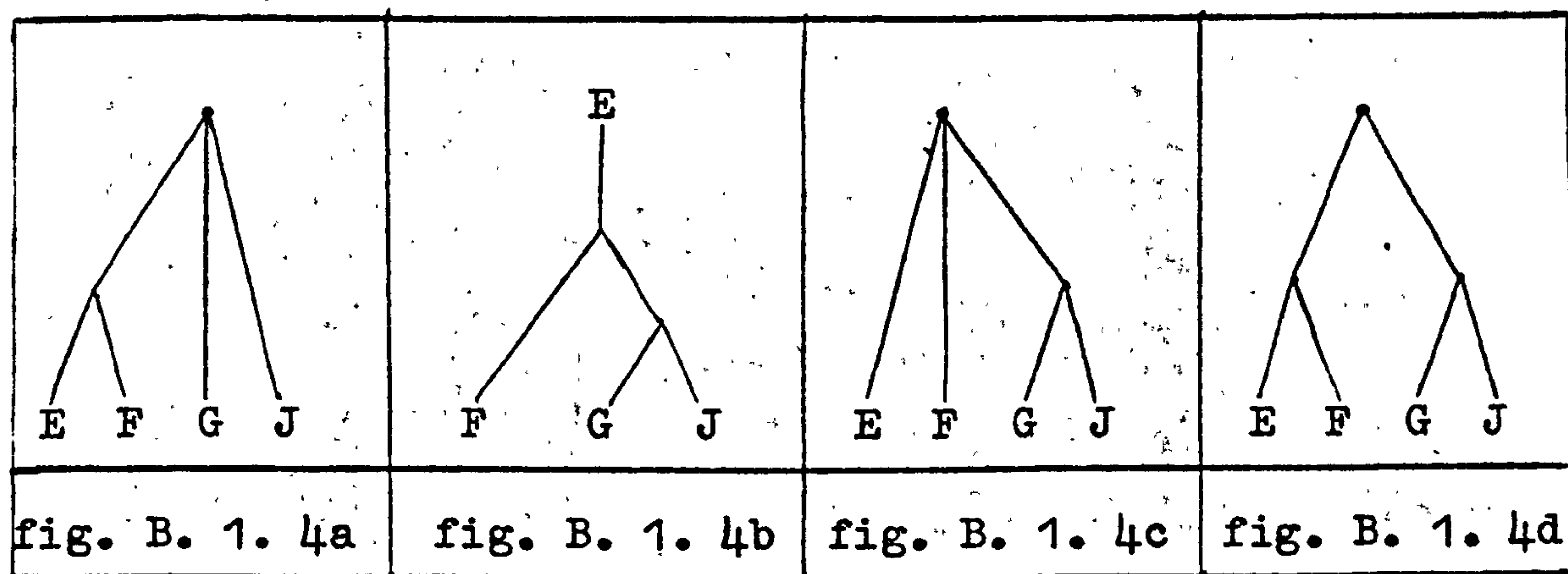
Lemma 3. Any split which can be generated by cutting the arcs of the stemma could theoretically occur in the construction domain, and would do so if the topmost point ms in each of the sub-trees produced (except that which contains the archetype) committed an error at the same point in the text.

The proof of the lemma follows readily from assumptions (IV) and (V).

Our next step towards proving the ambiguity theorem is to introduce the idea of topological equivalence. If two figures are such that one can be continuously distorted into the other, where the distortions may include rotation, stretching, bending of lines, etc., then the two are said to be topologically equivalent (t.e.). Thus fig. B. 1. 3a and fig. B. 1. 3b are t.e. to each other but not to fig. B. 1. 3c, since no distortion could turn the four arcs which in a,b meet in the centre, into the three of c:



Distortions of this sort may also be applied to a stemma, and thus we can envisage stemmata which do not represent identical textual histories but are nevertheless t.e., such as the following:



These differ only in the location of the topmost point, representing the archetype. Varying the topmost point is the only meaningful distortion which one can perform without the figure ceasing to be a stemma. That point, from which one "picks up" a stemma in order to obtain another which is t.e., may be any of the point mss, whether extant or not, or may lie along any of the arcs. Altogether

there may be as many as $4E-5$ stemmata which are t.e.¹

The interest of such a family of t.e. stemmata lies in Lemma 4, from which the theorem of ambiguity will follow immediately:

Lemma 4. Let S be any stemma which sketches the history of a textual tradition. Then the set of splits which occur among the extant mss could theoretically have occurred had the textual history been different, as sketched by any other member S' of the family of stemmata topologically equivalent to S .

Proof. To every arc in S there corresponds an arc in S' , with one minor complication. This is that if S is two-branched, there will be two arcs leading down from the archetype, but these will fall into just one with any other orientation; thus the path² from α to D , "bent" in S , becomes "straight" in S' . To get over this, we shall make a change in our terminology, to hold good over the proof of Lemma 4, viz: a path such as that from α to D shall be deemed to be one arc - admittedly bent, but nevertheless one - and thus the correspondence between arcs is complete.

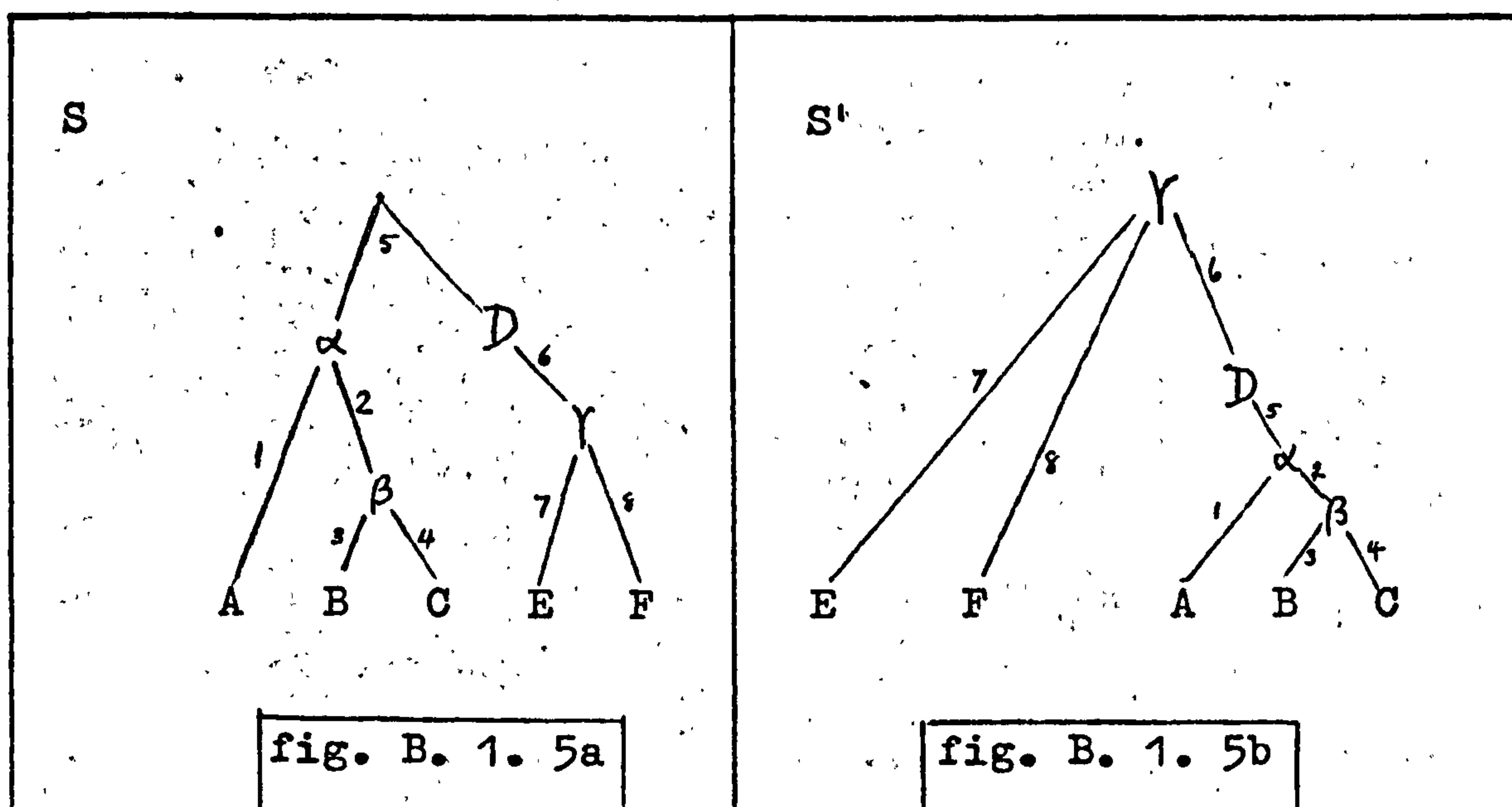
Consider any k -way split occurring in the text. It is possible to cut $(k-1)$ arcs of S so as to divide S into k sub-trees, and the mss into the appropriate k classes (Lemma 2). To every arc of S , there corresponds an arc

1. This can readily be proved by mathematical induction. The maximum ($4E-5$) is found in the case of a fully binary tree.

2. See fig. B.1.5 below.

of S' ; and by cutting the $(k-1)$ corresponding arcs of S' we again divide the mss into the same k classes. As this k -way split can be generated by cutting the arcs of S' , it could theoretically have occurred in the textual history represented by S' (Lemma 3).

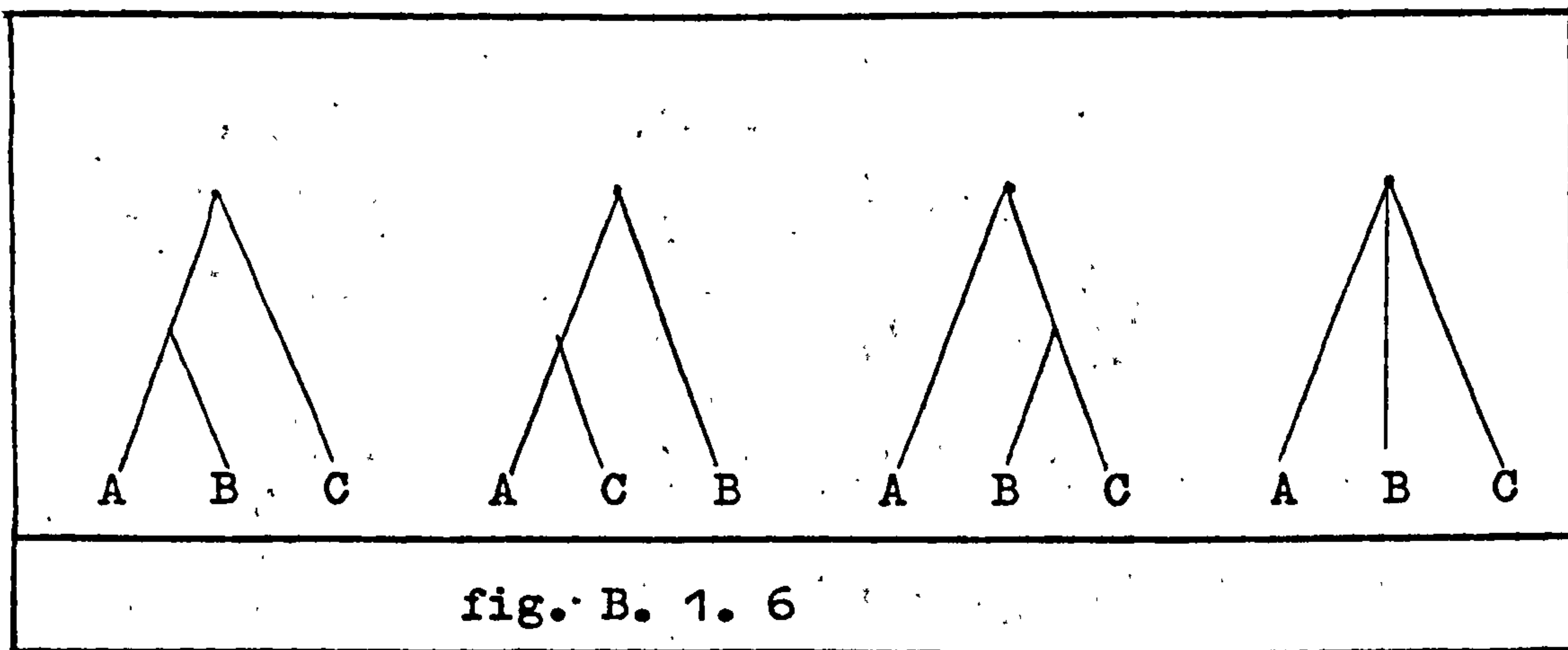
This can be illustrated by the two t.e. stemmata S and S' of fig. B. 1. 5., where extant mss are represented by Latin letters, lost point mss by Greek letters, and arcs by numbers.



The split $A:BC:D:EF$ may be derived from S by cutting arcs 1,2,6; it would result if errors were committed in the writing of A , β and γ at the same point in the text. However, the same split could also be generated by cutting arcs 1,2,6 of S' . In a tradition whose history was sketched by S' , errors committed in the writing of A , β and D would result in that split, viz $A:BC:D:EF$.

It will now be clear that if we are given a list of the splits produced by a ms tradition whose history is represented by a stemma S , and we have no further information, then there is no way of knowing whether this list has come from S or from a t.e. stemma S' . This proves the theorem of ambiguity, that the stemma cannot be uniquely defined through the objective evidence alone.

This fact was known to Dom Quentin¹ though he did not seem to understand its implications fully (Froger, p. 50). It is also implicit in Greg, who was well aware that one series of splits could well derive from more than one stemma; see his discussion of the "ambiguity of three texts" (pp. 21f and 45ff.), viz the fact that when we have three mss ABC, the occurrence of splits $A:BC$, $AB:C$, $AC:B$, $A:B:C$ does not specify which is the true stemma out of:

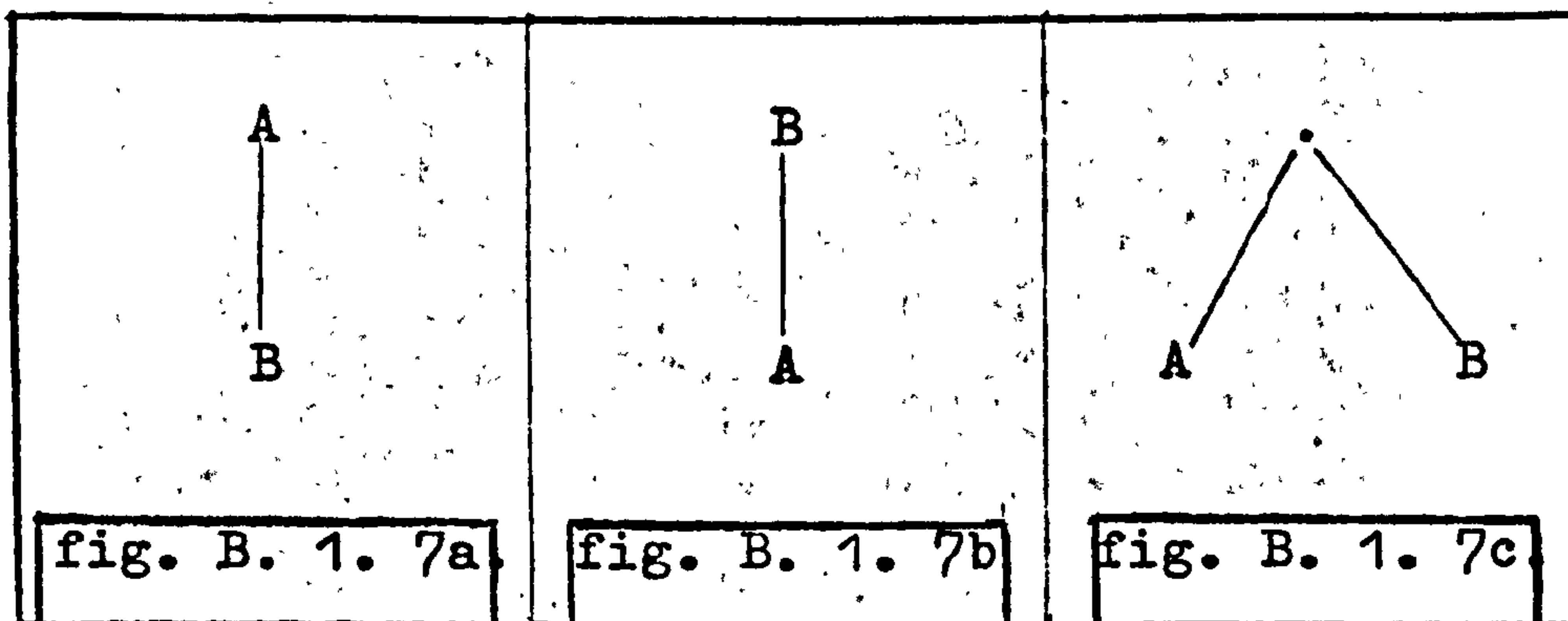


J. Bédier too, in his attack on the stemmatic method,²

1. See his description of his method (Mémoire, pp. 209-230). The point is brought out rather more clearly in his "Essais de critique textuelle", Paris 1926, especially pp. 80ff.
2. "La tradition manuscrite du Lai de l'Ombre: Réflexions sur l'art d'éditer les anciens textes", in Romania (1928), pp. 161-196, 321-356. See pp. 166 f and p. 338, where he states: "...le schéma reste malléable comme le plomb."

pointed out that the ms groupings were consistent with more than one possible stemma. However, neither specified the topological relationship between these alternative stemmata. The credit for having elucidated this whole principle must go to Froger; his proof, however, is confined to two-way splits, and does not dispose of the possibility that more complex splits may distinguish a stemma from one which is t.e. thereto.

How are we to deal with this ambiguity? If we can discover, on intrinsic grounds, which of the alternative readings is the oldest, then we can go some way towards this end. Thus, suppose two mss AB, which diverge. No "objective" data will enable us to choose between the three stemmata of fig. B. 1. 7. But if A bears errors not found



in B, and B similarly has an error where A's reading is sound, then both (a) and (b) are excluded by assumption V. It will be seen presently that the distribution of errors can enable us to choose uniquely between the possible stemmata, if we are prepared to make additional assumptions.

Thus we must bring in "subjective" questions sooner or later. This brings us to the parting of the ways between Maas on the one hand, and Greg and Froger on the other. The former brings in from the start the rightness or wrongness of the readings, and works with common errors. The latter leave such considerations until later, because it is possible to derive a stemma compatible with a given list of splits, by "objective" considerations alone; only then, when we have rather more firm ground under our feet, need we proceed to "subjective" questions, in order to choose the appropriate member of the family of t.e. stemmata.

Let us first develop the "Maasian" approach. Consider a two-way split. where we are confident that one particular reading is original and the other incorrect. The extant mss will be divided into two complementary sets; that which consists of the mss carrying the erroneous reading may be termed the "error set". The error itself arose in a single ms, which is a common ancestor of the error set (Lemma 1). All the other extant mss have the correct reading (V), and are therefore not descended from that ancestor, which is thus an xca of the mss of the error set. It follows that every error set in the construction domain indicates that that set of mss has an xca (Lemma 5).

To have a complete list of which sets of extant mss have an xca is to know the stemma. We may in fact set up the following existence theorem:

From the list \mathcal{L} of error sets, we may draw a stemma representing a textual history which would yield all the

error sets of \mathcal{L} and no others. This stemma is the only one to fulfil that requirement.

We first prove two propositions on the error sets.

Lemma 6 Let E_1 and E_2 be two error sets, derived from considering two passages in the construction domain. Then either E_1 and E_2 have no ms in common (or, in the language of algebra, are disjoint), or one of them wholly includes the other.

Proof. The mss of E_1 have an xca, say e_1 ; we may define e_2 similarly. Then there are three possibilities: Either e_1 is an ancestor of e_2 , in which case every descendant of e_2 is also a descendant of e_1 , whence E_2 is included in E_1 . Or e_2 is an ancestor of e_1 , with the converse effect. Or neither is an ancestor of the other, in which case no descendant of e_1 is also a descendant of e_2 (II, III), and the sets E_1 and E_2 are disjoint.

Our second proposition on error sets is:

Lemma 7 No two extant mss behave in an identical fashion over all the error sets derived from the construction domain.

This lemma cannot be proved by assumptions I-V alone. We need a further assumption, compounded of three postulates:

VIa. Every copyist commits errors of transcription, some of which occur at points of the text where no other copyist had hitherto committed an error.

VIb. Our construction domain is large enough for us to be confident that it contains, for each copyist, at least

one passage where he committed an error, and no other copyist deviated from his own exemplar, either to correct that error, or to commit independently that same error, or otherwise¹.

VIc. Our knowledge of the intrinsic criteria is good enough for us to be able to identify which of the variants is correct in each passage of the construction domain¹.

Proof. Let A, B be two extant mss. Then the relation between them must be one of the three possibilities of fig. B. 1. 7. In cases (b), (c), there will be at least one passage in the construction domain where we can perceive an error which is found in A but not in B, and where there are only two alternative readings. Thus there will be error sets in which A appears but B does not. Case (a) is similar to (b).

We may now prove:

Lemma 8 The complete list of error sets is also a complete list of those sets of extant mss which have an xca.

Proof. Let the two lists be \mathcal{L}_E and \mathcal{L}_X . Any error set has an xca (Lemma 5); thus any member of \mathcal{L}_E is also a member of \mathcal{L}_X . Conversely, let us consider any set of mss which has an xca, which we may call ξ . Then there will be at least one passage in the construction domain where an error was committed by ξ , and was transmitted to all its extant descendants (V, VI) and to no other ms (IV).









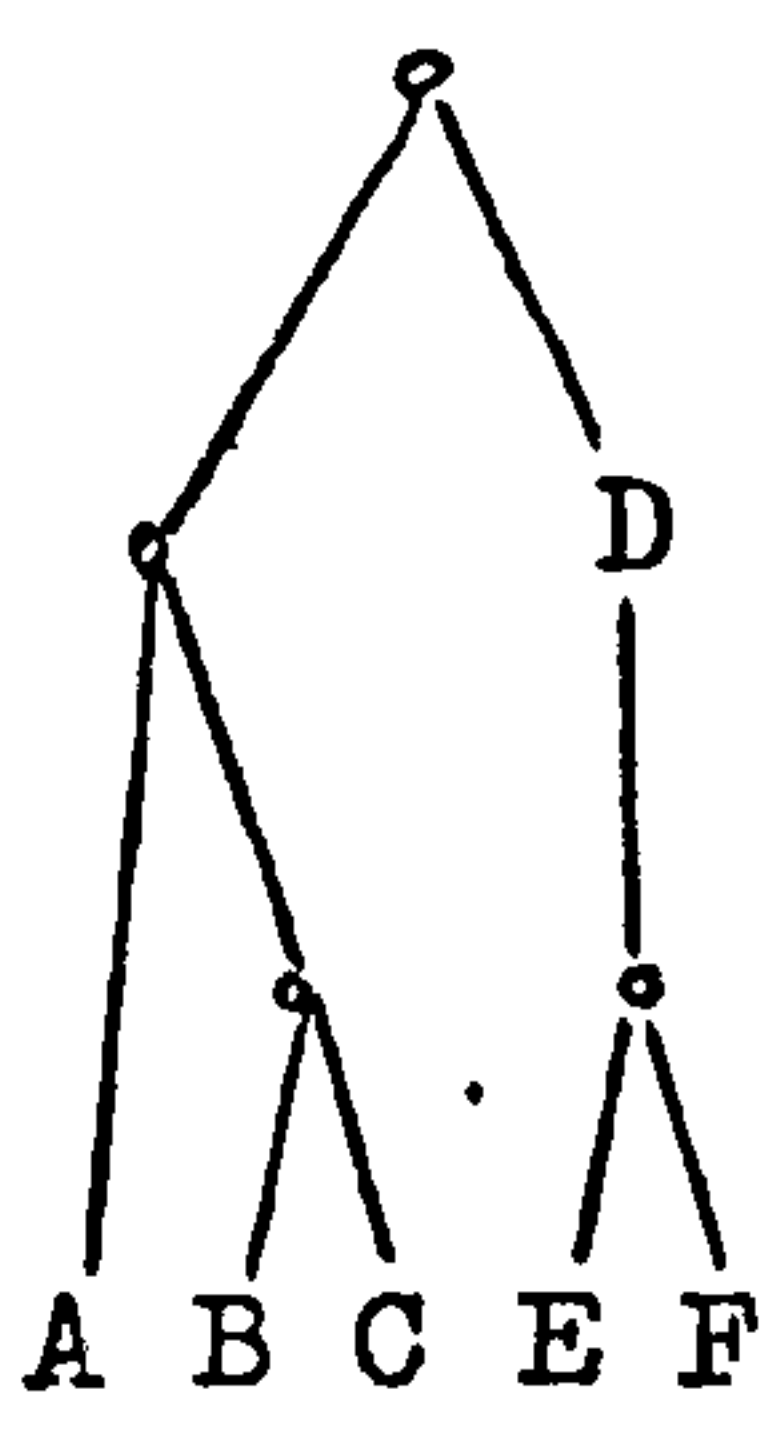
1. We do not always require so strong an assumption; see below.

This will yield an error set, consisting of all extant descendants of ζ . Therefore any member of \mathcal{L}_X is also a member of \mathcal{L}_E . Thus the two lists are identical.

The stemma may be drawn from the complete list \mathcal{L} of error sets in the following way; fig. B. 1. 8. will serve as an example. Let E_1, E_2, \dots be the n error sets, and let E_{n+1} be the set of all the mss. Let us represent each of these sets by rectangles, putting that of E_{n+1} at the top and putting the others below according to their size (fig. B. 1. 8b). Let E_i be any of the n error sets. We define E_j as the smallest of the n remaining¹ sets to contain E_i . This E_j is uniquely defined; for (a) E_i is contained in at least one of the n remaining sets, viz E_{n+1} ; and (b) if E_i is contained in two sets, say E_k and E_l , then E_k and E_l cannot be disjoint, so one must be contained in the other (Lemma 6), and there is no doubt which is the smaller. We now join the rectangle representing each E_i to the corresponding E_j (fig. B. 1. 8c). The resulting diagram will be a tree. Let us now change the naming of the rectangles by removing from each set E_i any ms which also occurs at a lower level; we thus obtain a reduced set e_i (fig. B. 1. 8d). Then every set e_i will either be empty or contain only one ms. For, let AB be two extant mss which occur in the set E_i and in no smaller set. Then the construction domain contains no passage

1. viz the $(n-1)$ remaining error sets, plus E_{n+1} .

where one has an error not in the other. But no two extant mss have identical readings throughout that domain (Lemma 7); therefore no set e_i will contain more than one ms. Thus there will be exactly one location for each ms; and the point associated with the set e_i will represent the xca of the mss of the error set E_i . Any of the error sets observed will be obtained if we suppose an error in the appropriate xca; and no others can be produced (IV, V). Finally, this stemma is unique; for

List of error sets			S
A ABC B BC C DEF E EF F	   	   	
fig. B.1.8a	fig. B.1.8b	fig. B.1.8c	fig. B.1.8d

any other stemma will differ in its complete list of xca's, by omission or addition or otherwise, and this will cause a corresponding difference in the complete list of error sets. This concludes the proof of the existence theorem.

Thus the "Maasian" method involves the choice of a construction domain over which IV, V and VI hold good. We

may assume provisionally that our domain will be large enough for (VI) to obtain - unless we have grounds for believing otherwise (e.g. if intrinsic considerations only seldom suffice to identify the correct reading; cf p. 7:2). To fulfil IV and V, we must find a set of passages in which some of the mss have an error which (i) could not have been committed by two or more scribes independently, (ii) could not have been removed by conjecture from an ancestor of any ms which has the correct reading. We have already assumed, by attempting to apply the stemmatic method, that contamination played no part. There are unlikely to be more than a handful of passages where we can be reasonably confident of both (i) and (ii). This creates a danger of basing the stemma on too small (and too personal) a selection of errors. However, there are ways of dealing with this. If, for example, we find the same set of mss agreeing in error in many passages, we may provisionally accept that set as an "error set". If a number of error sets can be identified in this way, and the sets are found to fulfil the "inclusion or disjunction" condition of Lemma 6, then a stemma based on that data will merit confidence.

An alternative and in many ways preferable method is that of Froger and Greg, who take advantage of the fact that a stemma can be deduced from the objective evidence alone (i.e. without appealing to VIc). Thus we may divide our task into two stages, viz recovery of a tree, and orientation. The choice of a construction domain can then be made on objective grounds (see below); and only in the last stage,

orientation, need we concern ourselves with the authenticity of readings.

Let S be the true stemma which we are trying to recover, and let one of our extant mss be A . Then there will exist a stemma S' , t.e. to S , which has A as its topmost point. Our theorem of ambiguity tells us that we cannot distinguish between S and S' on the objective evidence. Let us therefore pretend, for the moment, that our data came not from S but from a hypothetical textual tradition whose history is represented by S' . Then whenever we find a two-way split, there is no need to use intrinsic grounds in order to identify the "original" reading; for A is now our "original", and those mss whose reading is not that of A constitute the "error" set. Thus we may construct S' by the method given above for constructing S from the error sets; and then a consideration of the true error sets will enable us to orientate the tree correctly.

This is, in essence, the method of Froger. However, we have to verify formally that the procedure for reconstructing S' is justified on the basis of assumptions I-V, VIab, applied to S (not S').

Consider any two-way split. Then that set of mss which does not have the reading of A may be defined to be the variant set. We now prove a result on variant sets, corresponding to Lemma 6 on error sets:

Lemma 6a. Let V_1 and V_2 be two variant sets, derived from two passages where I-V obtain. Then either

V_1 and V_2 are disjoint or one includes the other.

The proof necessarily involves the mathematical notation of set theory. In addition, let us use the symbol $*$ so that $V_1 * V_2$ means: "either V_1 and V_2 are disjoint or one includes the other".

In either passage, there are two alternative readings, but we do not know which is correct. Thus V_1 may be identical either with E_1 or with its complement \bar{E}_1 . Of course the error sets still exist, and still satisfy the relation $E_1 * E_2$, even though we have chosen not to identify which reading is an error. We now shew that $(E_1 * E_2)$ implies $(V_1 * V_2)$, whichever ms is chosen as A.

Proof We may distinguish three possibilities:

Case 1 $V_1 = E_1, V_2 = E_2$. Proof trivial.

Case 2 $V_1 = E_1, V_2 = \bar{E}_2$.

$$\therefore A \notin E_1 \text{ and } A \in E_2. \quad \therefore E_2 \not\subset E_1.$$

But $E_1 * E_2$;

$$\therefore \text{either } E_1 \subset E_2 \text{ or } E_1 \cap E_2 = \emptyset.$$

$$\text{Now } (E_1 \subset E_2) \Rightarrow (V_1 \subset \bar{V}_2) \Rightarrow (V_1 \cap V_2 = \emptyset) \Rightarrow (V_1 * V_2)$$

$$\text{and } (E_1 \cap E_2 = \emptyset) \Rightarrow (V_1 \cap \bar{V}_2 = \emptyset) \Rightarrow (V_1 \subset V_2) \Rightarrow (V_1 * V_2).$$

Case 3 $V_1 = \bar{E}_1, V_2 = \bar{E}_2.$

$$A \in E_1, A \in E_2 \quad \therefore E_1 \cap E_2 \neq \emptyset.$$

But $E_1 * E_2$;

\therefore either $E_1 \subset E_2$ or $E_2 \subset E_1$.

Now $(E_1 \subset E_2) \Rightarrow (\bar{V}_1 \subset \bar{V}_2) \Rightarrow (V_2 \subset V_1) \Rightarrow (V_1 * V_2),$

and $(E_2 \subset E_1) \Rightarrow (V_1 * V_2)$ similarly.

Two variant sets which satisfy the relation $V_1 * V_2$ may be said to be consistent¹. The relation of consistency may be used to guide us in selecting the construction domain. We may expect that those ms groupings which are found most frequently in the text will be those in which readings have been transmitted "simply", without the affiliations being obscured by conjecture, coincidental agreement, etc. Therefore, we list all the two-way split passages in the validity domain, and count how many times each different type occurs. We then sort the split types in descending order of frequency, and go through that list until we find a split type which is not consistent with those that have gone before. That point

1. This is Greg's term; for his definition, see p. 1:42. Froger also mentions this relation of either inclusion or disjunction (pp. 249ff.). He proposes that the variant sets be compared by computer. I doubt, however, whether a text which was so widely read as to survive in enough mss to make the use of a computer worth while, is likely to have been transmitted without extensive contamination and thus to be suitable for stemmatic treatment.

will be the "noise-level", below which we shall find relatively infrequent groupings which would yield contradictory results. We admit to the construction domain only those passages where the mss show a two-way split of one of the types above the noise-level. Examples may be found on pp. 2:40ff, 45ff (where the attempt to draw a tree is not successful), and p. 11:58 (where it is). The construction domain obtained by this method is likely to be larger than that to be obtained in "Maasian" analysis.

From Lemma 7, that no two extant mss behave identically over all the error sets derived from the construction domain, we immediately infer a similar proposition on variant sets (Lemma 7a). We may also set up:

Lemma 8a The complete list of variant sets is also a complete list of those sets of extant mss which will appear in S' to have an xca.

Proof (a) Let V be any variant set. Either the mss V are in error; then they have an xca (in S), and there exists in S a point ms P which is an xca of the mss V . Let l be the arc above that point ms; if S is cut at l , we obtain two sub-trees, containing V and its complement \bar{V} . In S' there is a corresponding arc l' ; let P' be the point ms at the lower end of that arc. If S' is cut at l' , we again have two sub-trees containing V and \bar{V} . As $A \in \bar{V}$, the mss V will all lie below l' (in S'), and will appear to have P' as an xca. Or the mss V have the correct reading. Then \bar{V} is an error set, and we may similarly define P , l , l' and P' .

for \bar{V} instead of V . By a precisely similar argument, the mss V will appear to have P' as an xca.

(b) Let X be any set of mss which appear in S' to have an xca. Then the mss X will appear in S' to have as an xca a certain point ms P' . Let l' be the arc immediately above P' , l be the corresponding arc in S , and P the ms at the lower end¹ of l . Then P will be (in S) an xca either of X or of \bar{X} . In either case, the construction domain will contain at least one passage where P has committed an error, and the mss X agree against the mss \bar{X} . By definition, X cannot contain A , and it will therefore be found in such a passage to be the variant set.

Just as we were able to justify the drawing of a unique stemma S on the basis of Lemmata 6-8, we may argue similarly from Lemmata 6a-8a to draw a unique hypothetical stemma S' . This is illustrated in fig. B. 1. 9, on the same textual history as fig. B. 1. 8; here S' is constructed on the basis of the two-way splits² alone:

-
1. The arc l may be a "bent" arc containing the archetype (cf p. 1:21), and in that case P may be taken indifferently as either of the two point mss at its ends. Thus, in fig. B. 1. 9, if X is (DEF), we have two choices for P .
 2. Note how the two error sets (ABC), (DEF) in fig. B. 1. 8 correspond to only one variant set (DEF) in fig. B. 1. 9. Cf. previous footnote.

List of splits		
A:BCDEF ABC:DEF ACDEF:B ADEF:BC ABDEF:C ABCDF:E ABCD:EF ABCDE:F		
fig. B.1.9a	fig. B.1.9b	fig. B.1.9c

Finally we must decide where the topmost point should in fact be taken. The stemma may theoretically be "suspended" from any of the point mss, or from a point along any of the arcs. There are $(2p-1)$ possibilities, if S' contains p point mss. Now the topmost point represents the latest common ancestor of all the extant mss, which is termed the archetype. In order to choose, we bring in the question of correct and incorrect readings. Whenever we have an error set, the stemma may be cut - as it can at any point where we have a split (p. 1:17) - to yield two sub-trees, say T_1 and T_2 . They are separated by a single arc, which we define not to be contained in either sub-tree. All the extant mss in T_1 will have the error, and those in T_2 the correct reading. Suppose that we suspend the stemma from some point in T_1 , obtaining an archetype α . Then α will be the latest common ancestor of all the extant mss of T_1 . Therefore α itself must have carried that error

(Lemma 1). However, all the mss of T_2 are descended from α , and those that are extant have the correct reading. This contradicts (V); and so any point within the sub-tree T_1 can be ruled out as a possible site for the archetype. By considering different error sets, we eliminate different regions of the stemma S' . We may find that a unique site is determined; for example, if we obtained S' of fig. B. 1. 9c, and could point to an error in ABC absent from DEF, and to another in DEF absent from ABC, then the identification of these two errors alone would suffice for us to recover the true topmost point. (To recover S by the method of common errors, however, involves nine error sets, i.e. we must be sure which is the correct reading in at least nine passages.) Even if the archetype cannot be located uniquely, we may now be able to reduce the original $(2p-1)$ possibilities within comparatively narrow limits; such a result would be of value, but it could not have been achieved by the former method.

Thus some intrinsic information is still needed for us to determine a unique site for the archetype. However, in that this latter method (i) may enable us to recover S with the help of relatively little intrinsic information, which in particular will often be far less than what is stipulated in VIc, and (ii) allows us, if such information is insufficient, to make the most of what we do know - it has the advantage over the approach based on common errors.

Having derived the stemma, we must now apply it to determine the text of the archetype. The rule is that a reading common to two mss AB was also the reading of their latest common ancestor, say α . For, let us suppose otherwise: Either AB have a correct reading, and α had an error; then (V) is twice violated. Or AB bear the same error, which was not present in α ; then AB have the same error independently, against (IV). Thus a reading shared by two mss whose latest common ancestor is the archetype (e.g. AF in fig. B.1.5a) is also the reading of the archetype.

Let us formally define as the "application domain" the set of passages over which we shall use this rule to deduce the reading of the archetype. It would at first seem that IV and V had to be satisfied throughout that domain; but in fact we lose little by dispensing with V. For, suppose two mss AB, whose latest common ancestor is the archetype ω . What if we were wrongly to assume V? It might happen that in a certain passage, ω - which is usually not the original itself but a copy - had the corrupt reading ρ' , and AB had restored the correct reading ρ by conjecture. Then we would mistakenly attribute ρ to ω , instead of stating that the archetype was corrupt and proceeding accordingly (see below). However, our false deduction, that the correct reading ρ was in ω and hence presumably in the original, merely short-circuits the process of identifying the correct reading and can do little harm. Hence the application domain

requires only IV - in addition to I-III, which are needed for the stemma to be valid.

We note that even if the stemma were known independently (e.g. if it could be reconstructed from a sure basis of external data), the application domain would still need I-IV. When we cannot be certain about IV, we may speak - for the first time in this Chapter - in terms of likelihood rather than certainty, stating e.g. that as the agreement of certain mss is unlikely to be coincidental, it is likely to represent the reading of the archetype.

Sometimes it will not be possible to fix the reading of the archetype in this way, e.g. in fig. B.1.5a if ABC agree against DEF. We must then choose between the alternative readings on linguistic grounds ("selectio", cf Maas p. 13).

We have shewn (pp. A:53 ff.) that the archetype will not usually be the original itself; there may therefore be errors in the recovered text of the archetype. In such a case, we must resort to conjectural emendation (Maas, pp. 9ff).

This concludes our exposition of the theory of the stemmatic method. Table B.1.1 recapitulates the assumptions required:

TABLE B.1.1

<u>Assumption</u>		Whether required for domain of		
<u>No.</u>	<u>Brief statement</u>	<u>Validity</u>	<u>Construc- tion</u>	<u>Application</u>
I	unique original	/	/	/
II	unique exemplar	/	/	/
III	no subsidiary sources	/	/	/
IV	no coincidence in error		/	/
V	no successful correction		/	
VIa	all copyists err		/	
VIb	some errors peculiar to each copyist		/	
VIc	our "intrinsic" judgment		/	

Let us compare briefly the assumptions set up by Maas, Greg and Froger. All three aim to reconstruct a uniquely defined original text (I).

Maas states (p.6) that his treatment requires us to assume "dass die seit der Hauptspaltung geschehenen [i.e. subsequent to the archetype] Abschriften immer nur je eine Vorlage wiedergeben..., dass dagegen jeder Schreiber bewusst oder unbewusst von seiner Vorlage abweicht (Sonderfehler begeht)". This explicitly gives III and VIa, and implies II. These are the conditions set up for the application domain. Of the passages from which the stemma is to be

reconstructed, more is required. In Appendix I, Maas points out that the errors to be used must be suitable. Two types of error are considered:

(a) An error which is found in ms A but not in B, and could not have been removed by conjecture¹, shews that B does not depend on A ("Trennfehler", cf V, VIc)².

(b) An error which B and C have in common, and could not be due to coincidence, shews that BC have a common ancestor ("Bindefehler", cf. IV, VIc).

But Maas does not state that the construction domain must consist of passages where both IV and V hold, i.e. that we require errors which are simultaneously (a) and (b). Thus a rigorous justification of his method is not offered, although the method itself is expounded with clarity.

Greg postulates II, III (p. 2n.); and VIa (p. 9). He also assumes (p. 9) that "of the variants introduced in any transcript, some will persist through subsequent transcriptions, while others will undergo further variation"; the first part of that assumption is included in VIb. He mentions IV, V, in that he admits the possibility of their being unfulfilled ("convergent variation", p. 11). His touch-stone for selecting those groupings which are of

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1. Maas would regard a case of successful conjecture as "untypisch" (p. 9).
 2. His interest in (a) mainly concerns "eliminatio codicum descriptorum" (p. A:34). Unless a ms showed a Trennfehler against all earlier mss, Maas would eliminate it from consideration (p. 31).

"evidential value" (i.e. which are to be used in the construction domain) is "consistency" (p. 12); the conditions required for which, "appear to be satisfied if, and only if, given any two constant [i.e. frequently attested] groups, either these or their complements are either mutually exclusive or one wholly includes the other" (cf Lemma 6a)¹. However, no direct explanation of this requirement is given. No other assumption is invoked in deriving the set of various alternative stemmata, although Greg acknowledges that IV is relevant: "This assumption we do habitually make, and although it is not necessarily correct in any individual case, without it no inference as to the relation of the manuscripts would be possible" (p. 19n.) He too explains how his method for deriving a tree works, but not why it should be expected to work. The choice between the alternative stemmata (pp. 47ff.) is to be performed by inspection of passages where the original reading can be identified.

Froger speaks of an "arbre généalogique" (p. 11), implying II. He develops his theory in the context of "généalogie normale", the conditions for which are (p. 16): "Lorsqu' un copiste transcrit naïvement son modèle, il fait des fautes [VIa] ... Mais si son modèle a déjà des fautes ... ce serait une étrange coïncidence s'il en faisait aux mêmes endroits; il reproduit donc les fautes de son modèle [V], et en ajoute d'autres en des endroits différents. D'autre

1. It is a simple exercise in set theory to shew that the relation described here by Greg is equivalent to that of Lemma 6a.

part, quand deux ou plusieurs copistes travaillent indépendamment, ce serait encore une coïncidence s'ils faisaient les mêmes fautes, et les probabilités veulent qu'ils fassent des fautes différentes [IV] en des endroits différents [cf. VIb]." Thus we have VIa; V, IV (note that IV, V together include III)¹; and an assumption which is somewhat stronger than VIb. He develops on this basis a method for reconstructing a stemma S^h which is t.e. to the true stemma S , and then successively eliminating different regions of the tree so as to identify the true topmost point. It is this procedure, essentially, which has been set out above (pp. 1:30 f.). However, Froger does not shew rigorously how each step is justified, nor does he prove that the stemma thus recovered will be unique, with no two mss occupying the same position². He admits (p. 58) that "il est infiniment probable qu'il ne se rencontre jamais, dans la réalité, de généalogie absolument simple et normale"; but we are not told by what right we may apply the method - as he claims to have done (pp. 59, 276) - to passages in real textual traditions, where we cannot be certain that all his assumptions are fulfilled.

One final point deserves to be made. The charge has been brought against the stemmatic method, that it rests essentially on a circular argument³; for the rules which it

1. See p. 2:6.

2. In fact, on p. 85 he entertains the possibility that two or more extant mss may be found to have identical readings throughout - which contradicts VIa (see proof of Lemma 7 above).

3. e.g. by Hill, p.66: "Variants are first judged as right or wrong. Rightness or wrongness is then used as a means of establishing authority, which in turn is used to establish rightness or wrongness".

sets up to identify erroneous readings in what is claimed to be an objective way, follow from an outline textual history which itself depends on our earlier identifications of erroneous readings. But this accusation, I submit, is undeserved. The initial identifications of errors, in the construction domain, are assumptions which, taken in conjunction with I-VI, provide a basis for deducing a stemma and hence for arguing from the construction domain to other points in the text where we cannot confidently pick out the true reading. Once we have included among our fundamental set of premises the incorrectness of certain variant readings, the rest follows by a satisfactory process of logic; what we do proceed to question now, however, is the validity of assumptions I-VI, from which that logic sets out.

2. The Stemmatic Method in Practice

The method described in Chap. 1 will fulfil our need for an outline history of the text and for guidance in choosing between variant readings - if the basic assumptions are reliable. Now most of them are open to objection, theoretically; but they can hardly ever be tested directly in practice, in that they refer for the most part to the behaviour of the scribes of mss which are now lost and the former existence of which we will probably not even apprehend.

It is this which will determine the manner of our decision on the validity of the stemmatic method, when we come to study any particular text. If we were then to find that the assumptions were satisfactory in that they explained the observed facts and led to consistent results, then it would seem churlish to reject those results on the grounds that they rested on postulates which had not been proved. If, on the other hand, the attempt to apply the methods of Chap. 1 were to lead to results that were palpably unsatisfactory, then we could infer that, for that particular tradition, our set of assumptions was seriously unrealistic. To that extent they can be indirectly disproved; but apart from that, they are virtually untestable.

Hence there are two approaches whereby we examine in this Chapter the practical usefulness of the stemmatic method:

- A) We consider each of the six assumptions in turn, to see in what respects it may be inadequate, what

danger lies in supposing it to be true when it is not, and what precautions can be taken against that danger.

- B) We attempt to apply the method to particular traditions, so that we can see whether it ever leads to unsatisfactory results, and if so, in what manner. Two texts have been selected for this purpose, neither of which has yet been found amenable to a stemmatic approach, viz the De Unitate of Cyprian, and the Persae of Aeschylus.

Before going any further, we must find an objective criterion for deciding what would constitute an unsatisfactory result, in the sense of B. The stage at which any serious shortcomings are first likely to betray themselves is the construction of the stemma. How then can we decide objectively whether the textual data gives satisfactory results in the construction process?

Let us adopt the method of Froger (pp. 1:30 ff.), and select our construction domain accordingly (pp. 1:33f.). This allows us to work with the more frequent groupings, down to a noise-level below which the groupings are inconsistent. The construction domain thus obtained will accord by definition with Lemma 6a (i.e. all the groupings will be mutually consistent); but it would nevertheless yield an unsatisfactory result if either of the following symptoms appeared:

(i) A large proportion (say 50%) of the passages where the mss show a two-way split fall outside the construction domain. This would mean that the stemma to be obtained from the construction domain will fail to account for the behaviour of the mss in the majority of the variant passages.

(ii) There is no well-marked division between the "constant" groupings, which we admit to the construction domain, and the others. Thus, if we were to accept a grouping which is attested five times, and to reject another which occurs four times and conflicts with the former, then we could hardly be confident¹.

Consider for example a tradition with four extant mss ABCD. If we found the splits AB:CD (10 times), AC:BD (9 times), AD:BC (8 times) and no others, then our construction domain would consist of the ten passages where AB agree against CD; but it would be found unsatisfactory on both counts. These two criteria, then, are to be used in our "practical" tests(B).

We now examine each assumption in theory (A), and bring in practical material (such as B) at an appropriate point.

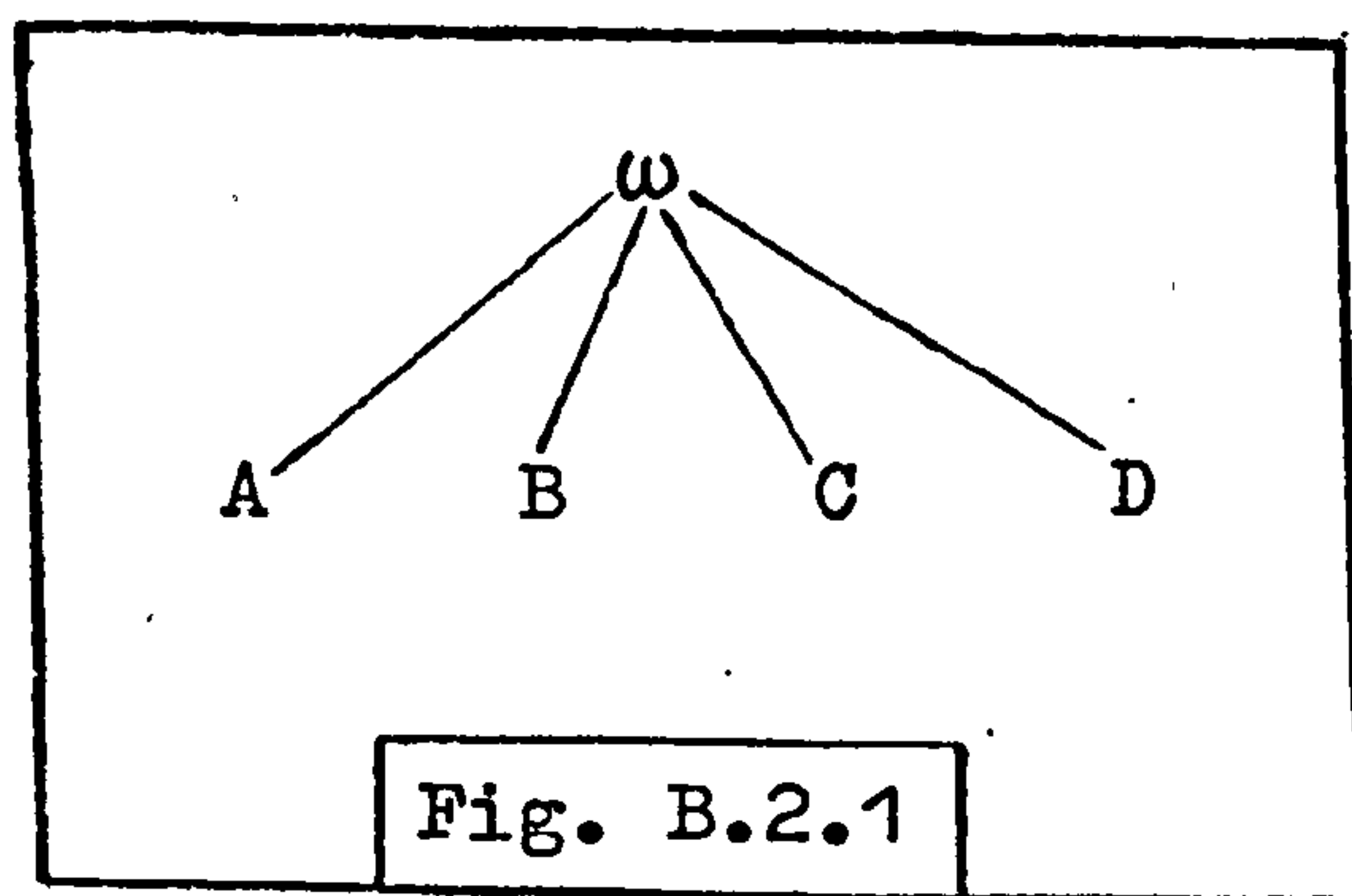
Our discussion begins with assumption (I): unique original. We have already remarked that this does not allow for cases wherein two or more alternative readings go back to the author himself. Thus H. Kantorowicz² points

1. The construction domains selected on pp. 2:42, 46 are unsatisfactory on this count.

2. "Einführung in die Textkritik", Leipzig 1921.

to the situation (pp. 5f.) where an author issues a revised edition of his work; the mss may then come to offer two (or more) different readings, each of which derives from the author and can thereby claim to be "original". In the context of the Bible versions, a similar position would follow from the "Kahle view" (p. 8:1).

In a domain over which (I) was repeatedly false, not only would we find a stemma inadequate, in that it can yield only one original reading; the construction might also be impeded. Thus, consider the situation of fig. B.2.1, where ω is an original presenting in several passages two alternative readings, and ABCD are independent copies. It may happen that at some points, AB agree in choosing one of the alternatives, whereas CD prefer the other, whence a split AB:CD; and that at other points we



find similarly the splits AC:BD and AD:BC. Suppose that the three types occurred with roughly equal frequency. Then, in coming to reconstruct the stemma from the variants, we would find the construction domain unsatisfactory, and reach an impasse.

However, the case of a multiple original is perhaps exceptional. It may indeed prove necessary, in extreme cases, to abandon the stemmatic approach; but usually a partial remedy will lie in excluding from our validity domain any variant which seems likely to be due to the invalidity of (I). Thus we shall often be able to assess, from the character of two alternative readings in the Septuagint, the respective likelihoods (a) that they represent two independent translations each made from a Hebrew source, and (b) that the divergence arose through inner-Greek scribal developments. Once this selection - if necessary - has been made, it is surely unlikely that the constant groupings, on which the stemma is to be based, will reflect a multiple original. Thus the stemmatic method is limited by (I), but not severely.

Assumption II, that every ms has a uniquely defined exemplar (i.e. main source), will readily be granted; for no matter how many mss a scribe consults in copying a given stretch of text, there must be one from which he deviates less than from any other, and that ms is uniquely defined as the main source or exemplar. It was this definition of the term "exemplar" that allowed us to develop the theory of Section A with reference to textual traditions in general, independently of any further assumption.

Our third assumption goes much further, in that it implies that the main source of each ms was the only source, and that the readings of a ms will not survive unless it leaves a surviving descendant. It will be recalled that

this assumption is required to justify the simplification of the full tree-diagram into a stemma (p. 1:11).

Our discussion of III is complicated by the relationship of III to assumptions IV and V. If a scribe S introduces a reading from some ms C other than his exemplar, then that reading is either the original reading or an error. In the former case, he will successfully correct an error in his exemplar, against V. In the latter, the same reading will be produced by two different copyists each of whom deviated from his exemplar, the two copyists being (i) S and (ii) the scribe who originated the error in C or a source of C; thus IV will be violated. As III is thus included in IV and V together, we shall not discuss separately the effects of the invalidity of III, but leave them till we come to IV and V.

The first casualty, if we reject III, will be the right to speak in terms of a stemma at all. A concept which will help us to appreciate this is that of the source-complex of the mss. We define the source-complex of a ms M to be the set of all mss from which M is derived, together with M itself¹. This source-complex will consist of every antecedent of M, direct or indirect, going back to the original, with the addition of M. For example, in the genealogy of fig. B.5.1 (p. 5:3), the source-complex of the ms R is the set of mss ACDEFGLNOPQR.

1. M itself is included by analogy with our definition of "ancestor" (pp. 1:13f.).

Similarly we may define the source-complex of a set of mss to include any antecedent of one or more members of that set. By the term "source-complex of the mss" or "source-complex" on its own, we shall mean the source-complex of the set of extant mss.

Under the conditions envisaged in Chap. 1, the source-complex will consist of the point mss together with the arc mss. Thus in the hypothetical tradition considered on p. A:30, in which we have 15 survivors out of altogether 77 mss, the source-complex is found to contain 28 mss (see fig. A.3). However, if we are studying a tradition in which it was not uncommon for scribes to use two or more sources, we shall have to think of a much larger source-complex, perhaps very many times larger than the set of extant mss itself.

There are several ways in which III may come to be violated. The most obvious is that a scribe has two (or more) exemplars before him when he first makes his copy¹. An eminently learned scribe - who might indeed deserve to be called an editor - may be acquainted with many of the mss available, and choose his readings eclectically². But the commonest cause of the invalidity of III is the correcting of mss which have already been written. Suppose that a scholar compares a ms M with another ms C. He may

1. The term "conflation" is used of this process.

2. This is called "editorial conflation" by Greg (p.57).

then introduce readings from C into M; perhaps he will delete some of the readings of M, perhaps he will make no deletions but content himself with inserting the reading of C in the margin or between the lines. In the former case, M will have passed into a second "state". M as corrected (which we may denote by M^2) will be a mixture of M in its original state (M^1) and C, just as if it had been the work of a scribe who had M^1 and C before him and followed sometimes one, sometimes the other. A ms may be corrected several times, and thus go through a succession of "states" (M^3 , M^4 , ...). In the latter case, M itself would remain intact, but a copyist who then transcribed M, augmented by marginal notes, might follow sometimes the reading in the body of the text, sometimes that in the margin¹. Thus all mss derived from M would appear to go back to a conflation between M and C. Such processes are called "correctional conflation" by Greg (p.56); more often they are referred to as "contamination" (e.g. Maas, p.8)². That the correction of mss was a common practice, may be deduced in many cases (e.g. that of Aeschylus or of the Peshitta) from an inspection of the extant mss themselves. Again, M.L.W. Laistner³ mentions (p. 208) that the ninth-century scholar Servatus Lupus explicitly recorded his eagerness "to obtain a second manuscript of some work that he already possessed in order to

1. See e.g. Greg, pp.56f. The possibilities at the stage of copying are considered in detail by M. Bévenot, "The tradition of manuscripts", Oxford 1961, p. 127.

2. Pasquali discusses at length the workings of contamination (pp. 146ff.).

3. "Thought and Letters in Western Europe", London 1931.

collate the two and improve his own copy".

Another way in which III may be contravened is by change of affiliation. Suppose that a scribe forsakes the exemplar he was using, e.g. because it is defective, and follows a second ms; he may continue using that second ms, or move on to a third, or go back to the first, and so on. Over any given domain, there will be one ms from which he has deviated least ("the exemplar", cf. II); but he will have taken over a great deal from subsidiary sources, even if he never used more than one ms at the same time. Thus the ms inter-relations need not be the same throughout the text under analysis. In an uncontaminated tradition, we would probably detect changes of affiliation, and divide the text into several domains which could be studied separately. However, if contamination has been extensive, we cannot expect to keep track of the changes which occur throughout the text. Firstly, any significant change in a scribe's policy will be tantamount to a change of affiliation, e.g. the decision to start using a subsidiary source while keeping in the main to his old exemplar, or to pay more (or less) attention to variants recorded in the margin of his exemplar. Secondly, the number of sources from which our extant mss are derived, i.e. the source-complex, will be very great, and a significant change of affiliation in any of them will mean a change in the textual history. As a number of such changes are likely to have taken place in the course of a given work, we must expect the textual history to be not only complex but also unstable. Some statistical

evidence on this point is offered on pp. 3:18ff.

The effects of the invalidity of III will be discussed presently. Meanwhile we turn to IV, which may be violated in the following ways:

(i) two or more copyists independently commit the same error;

(ii) a copyist uses a subsidiary ms over some portion of the text (e.g. through caprice, or where his main source is defective), and reproduces errors which he finds in that subsidiary ms but have never been in the main source;

(iii) the main source offers a reading which is original but presents a difficulty of some sort, and the scribe prefers a plausible reading which is offered by another ms (or by the margin) and is in fact no more than a facile emendation.

Of these three processes, (ii) and (iii) have already been discussed; but (i) deserves closer attention.

That different copyists sometimes commit identical errors is a fact that has been verified experimentally. Kantorowicz records (pp. 48f.) an experiment in which he gave instructions for a text unknown to him to be transcribed by a series of persons - the genealogy also being unknown to him - who were each to make slight alterations. The mss. were then brought to Kantorowicz,

who used the traditional methods to reconstruct a stemma and thence the original text. He was surprised to find that in about one-tenth of the variant passages, his choice of reading was incorrect; in each case, copyists from two (sometimes even three) different classes had independently committed the same error. Kantorowicz remarks, very properly, that coincidence in error can mislead us both in the construction¹ of a stemma and in its application (cf. p. 1:40)².

Another experiment was conducted by J. Stoll, who was investigating the psychology of scribal errors³. Stoll's work is not well-known among textual critics, and as his findings are of great interest I make no apology for offering here a brief account of them. Stoll prepared four passages, of which three were in German of differing styles and the other a series of nonsense words. He then had twenty-seven students copy them out, under identical conditions. The errors made by them were analysed. In many cases the same error was found in more than one script, and Stoll devoted a short chapter to this phenomenon (pp. 129f.). His explanation was that the vast majority of the errors had been found to be due to three factors, operating either singly or in combination, and that different people committed the same errors because they were subject to the same factors. These three are:

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1. This danger is considered in detail below.
 2. Pasquali (pp. 17f.) also mentions examples, from his own experience, of coincidence in error.
 3. "Zur Psychologie der Schreibfehler", in Fortschritte der Psychologie (1913), pp. 1-133.

1) Deviation in the direction of greater familiarity. A copyist may replace an element of his exemplar, e.g. a word or a syllable, by one which is more familiar to him. One might have thought that this proposition could hardly be tested, in that the relative degrees of familiarity between words etc. vary among different speakers; for example, the word "textual" will be more familiar than "textile" to a linguist but not to a tailor. However, there are well-marked general trends, which are reflected in the relative frequencies of linguistic elements as presented in a statistical word- or syllable-count based on a wide range of material¹. Many particular cases of this, e.g. trivialisation, are of course familiar to textual critics.

2) The tendency, when one is faced with two identical or similar elements, to suppress one of them. Thus three students wrote "quantitav" for "quantitativ", three wrote "Erober" for "Eroberer", and, in the nonsense passage, "hobfaupfjoch" became "hobfaupjoch" in two scripts and "hobfaufjoch" (cf. the phonetic similarity of b and p) in three. As the examples shew, this tendency goes rather further than what is generally thought of under the heading of "haplography".

1. By consulting Kaeding's "Häufigkeitswörterbuch", Stoll was able to prove his point.

3) Perseveration, in which an element which is visually or acoustically prominent "sticks in the mind" of the copyist, and in the copy makes an appearance at the wrong point. This phenomenon takes different forms, according as (i) the point at which the perseverated element wrongly appears is before (Vorwirkung) or after (Nachwirkung) the correct point, (ii) the perseverated element does or does not appear in its correct position as well, (iii) another element is or is not displaced. Thus four subjects wrote "Proßeß" for "Prozeß", nine put "nahmhaft" for "namhaft", and three replaced the nonsense word "drachpáz" by "drachpräz". Perseveration was found to affect not only words and letters but even parts of letters. This type of error is also well-known among textual critics¹, though perhaps not as much as (1) and (2); and its full implications are not universally appreciated.

I have attempted to summarise in Table B.2.1 the workings of (2) and (3). Here, abc...p each denote a chain of one or more linguistic elements; the symbol p is reserved for a perseverated chain; a' denotes a chain which is identical or similar to a.

1. Thus G. Kane, "Piers Plowman: the A version", London 1960, p. 121: "Mechanical errors of copying also occurred through 'attraction' to the whole or part of an adjacent or nearby word in the line being copied or in a nearby line. Presumably the scribe's attention either returned inadvertently to matter already copied, or was distracted by copy to come, his pen being misdirected accordingly".

TABLE B.2.1

<u>Symbolic representation</u>		<u>Example from Stoll</u>	
<u>Process</u>	<u>text</u>	<u>text</u>	<u>copy</u>
suppression	aba' →	quantitativ	quantitav
	aba' →	Revisionismus	Revionismus
Vorwirkung	abp →	Viertel	Vietel
	abp →	kurzt	kurtz
	abp →	namhaft	nahmhaft
	abp →	Prozeß	Proßeß
Nachwirkung	pab →	konsequent	konsekquent ¹
	pab →	legte	legtt
	p →	" _{am} ²	" _{amm}

Note: We often find that the two processes of suppression and perseveration combine, e.g. aba'cp → abpcp (as in: Kandidaten → Kandidaten), which combines the first and sixth changes in our table.

1. This type is comparatively rare.
2. in the nonsense passage.

Of all the errors observed in the experiment, Stoll found that only 5.3% could not be explained in terms of these three factors (p. 118). In any ms tradition, they must be counted as potent sources of error; and as all copyists are exposed to them in much the same way, it is inevitable that IV will sometimes be violated in this respect.

It need hardly be pointed out that Stoll's experiment is confined to unconscious or mechanical changes, which are not the only causes of variation in texts. Other factors, either material (e.g. blots on the page) or intentional (e.g. misguided emendation, changes on religious grounds) can also bring the same unoriginal reading into many scripts independently.

How is the stemmatic method affected if IV is false? We may answer this question in terms of error sets. According to the assumptions of Chap. 1, to every arc of the stemma there corresponds a two-way split; if an error is committed by any arc ms along that arc, or by the point ms at the base of that arc, then there will be produced an error set containing all extant descendants of that ms. Thus, it will be recalled, we may derive from the stemma a list \mathcal{L} of all the error sets that can occur. Now, if two mss, "belonging" to different arcs, commit the same error¹, then the two corresponding error sets will coalesce,

1. In the interests of simplicity, we first consider a case in which the assumptions of Chap. 1 are upheld everywhere else; the effects of their invalidity on a large scale are considered below.

giving rise to a new error set which combines the original two. Concerning this new error set, we consider three possibilities:

- (a) It too is a member of \mathcal{L} .
- (b) It is not a member of \mathcal{L} , but it is consistent with every member of \mathcal{L} .
- (c) It is inconsistent with one or more members of \mathcal{L} .

The corresponding outcomes are:

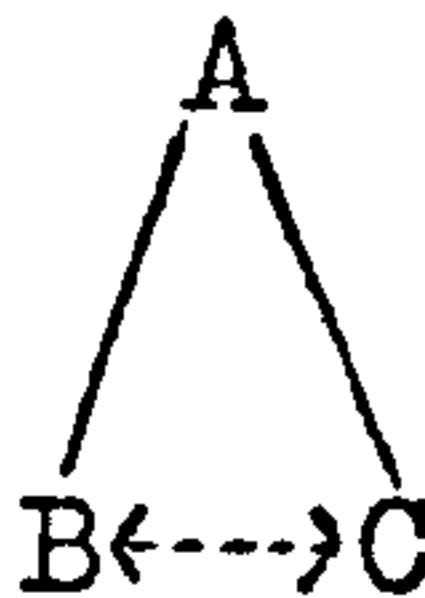

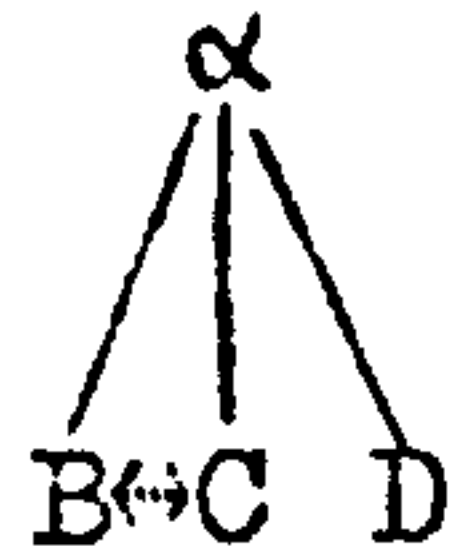
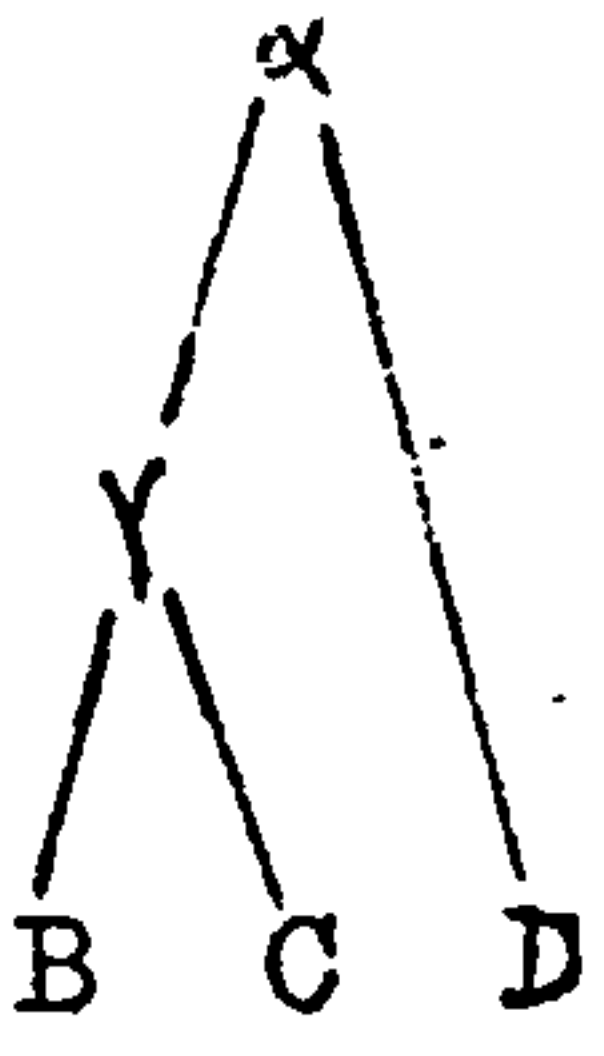
- (a) No substantial effect.
- (b) We shall derive an incorrect stemma.
- (c) Our attempt to construct a stemma will be impeded.

Let us now discuss these possibilities in detail.

(a) Consider a textual tradition whose history is sketched by the stemma of fig. B.2.2b(ii). Then \mathcal{L} will consist of the sets: B, C, BC, D. Suppose that B and C coincide in error. Then the combined error set, BC, is also a member of \mathcal{L} , and we shall imagine, wrongly, that their common error went back to γ . But as BC in fact have a common ancestor γ , little harm is done.

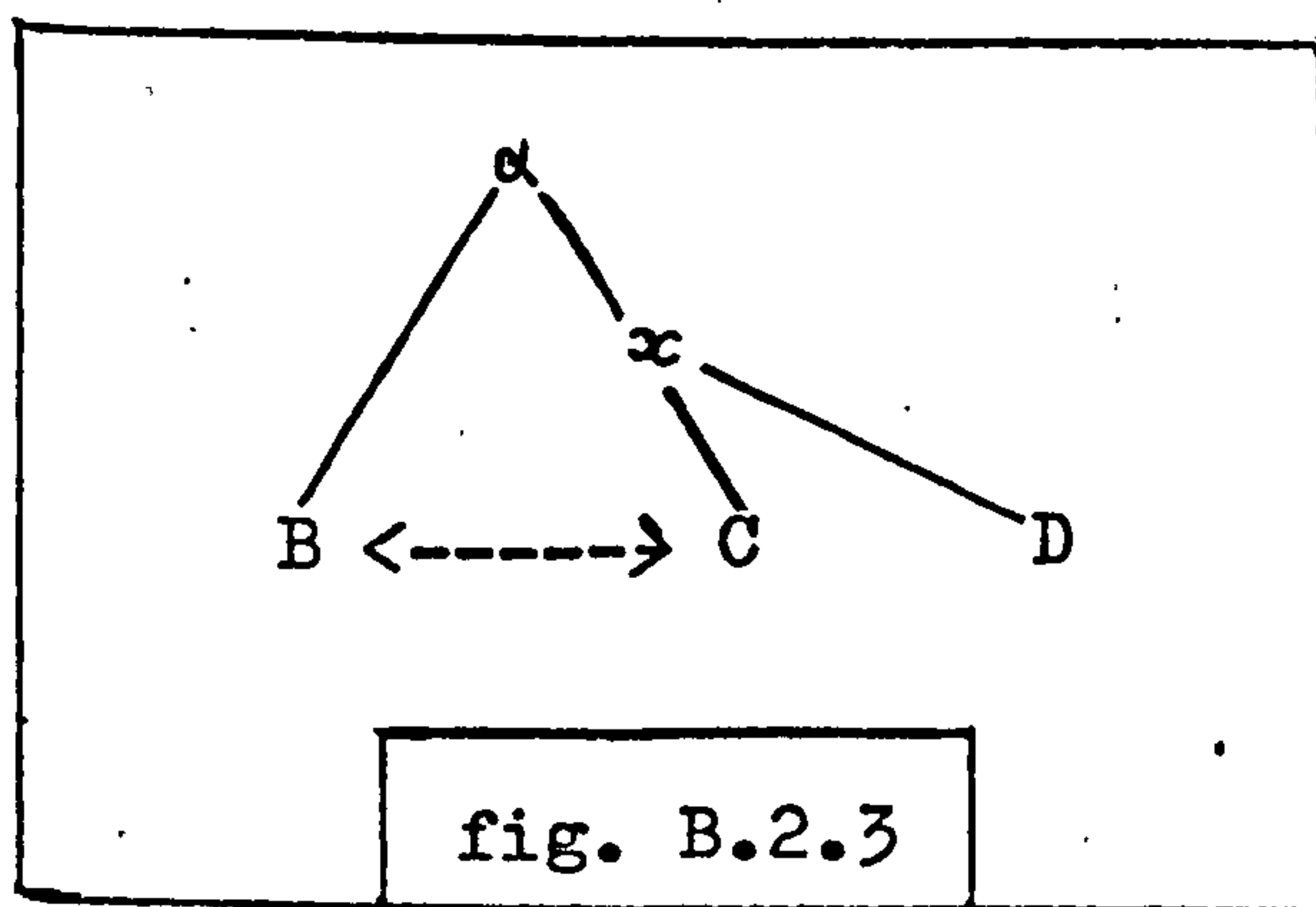
(b) The danger here is that we shall add the new error set to \mathcal{L} , and thus obtain a new list \mathcal{L}' from which a misleading tree would be derived. The additional item in \mathcal{L}' will lead us to postulate a "ghost" ms, which appears

to be an xca of the mss of this new error set and accounts for their common error¹. Examples are provided by fig. B.2.2, in which two different textual histories are considered (a and b); in either case, (i) gives the stemma as it really is, and (ii) shows it as it will appear to us. In both a and b, BC... are independently derived from their latest common ancestor, and share sundry errors absent from that ancestor (e.g. by coincidence or by transference of reading from B to C); this agreement in error is represented in the diagrams (i) by the symbol \longleftrightarrow . The resulting error set {BC} will prompt us to infer a ghost ms γ , which is misleading when we have extant the true latest common ancestor itself (fig. B.2.2a) or a third independent branch derived from it² (fig. B.2.2b).

			
fig. B.2. 2a(i)	fig. B.2. 2a(ii)	fig. B.2. 2b(i)	fig. B.2. 2b(ii)

1. A question connected with this is considered by Pasquali, Chap. II: "Ci fu sempre un archetipo?" In some traditions, there are a few errors common to all the extant mss, but they are all minor and could easily have arisen in two or more scripts by coincidence. We cannot therefore be sure that all our extant mss derive from an archetype, rather than having no common ancestor but the original itself.
2. There, two of the three independent branches will appear to fuse together. This danger was pointed out by Kantorowicz, who believed it to be responsible for the prevalence of two-branched stemmata (see pp. A:67ff).

(c) In this case our new list of error sets will not satisfy Lemma 6 or 6a, and we shall not be able to obtain a tree unless we have some means of weeding out the additional error set, which does not reflect a genetic relationship. An example is given by fig. B.2.3. Here \mathcal{L} consists of the error sets B, C, CD, D. If BC



were to coincide in error, a new error set BC would arise; this would be inconsistent with CD, and we would be unable to reconstruct the true stemma unless we could satisfy ourselves that CD was the "true" grouping and the error set BC was to be ruled out.

Before we proceed to possible remedies against the dangers of (b) and (c), we note the different circumstances under which each of the three possible outcomes will occur. Let vw be two mss which either are extant or have left extant descendants; let u be their latest common ancestor. Suppose that vw agree in an error which was not inherited from u . Then the three outcomes correspond to the following cases:

(a) u is "dead" and has no extant descendant apart from the descendants of v and w .

(b) u itself survives, and/or descendants of a third (or even a fourth, etc.) branch of u survive, but there is no extant descendant of u which branches off anywhere along the paths $u-v$ or $u-w$.

(c) any case not included under (a) or (b); see e.g. fig. B.2.3, where we have an off-shoot from the line leading down from α to C^1 .

The only criterion to which we may look, in order to distinguish the "true" groupings, is frequency. This rests on the assumption that it is more frequent for an error to be confined to the progeny of the ms in which it first arose, than for it to reappear in other branches of descent for whatever reason (e.g. that the latter have borrowed the incorrect reading under the false impression that it was original, or have committed the same error independently). Since coincidence in error and mistaken correction are not really typical cases, we may hope that the "untrue" groupings

1. The last two possibilities, of an incorrect stemma and of an obstacle to the construction of the stemma, are set out by Froger (pp. 107 ff.). He suggests, however, that what determines whether the outcome is (b) or (c), is the number of generations which elapsed between vw and u : "L'on voit les groupes entrer en conflit lorsque l'un des deux collatéraux (à plus forte raison l'un et l'autre) est séparé de l'ancêtre commun par un ancêtre intermédiaire[in which term an arc ms is also included]". But the deciding factor is not how many generations are covered in the lines $u-v$ and $u-w$, but whether there survive any intermediaries along, or off-shoots from, those lines. We are concerned with the degree of affinity between u , v and w , not (as Froger suggests) absolutely, but relatively to whatever other mss survive.

- whether they will tend to falsify or to impede the construction of the stemma - will all come out below the noise-level. In practice, however, we can never test the effects of the invalidity or otherwise of IV in isolation; for V is also suspicious and can also mar the construction of the stemma.

Assumption V may be invalidated by any of the following means:

- (i) successful conjecture;
- (ii) a copyist uses a subsidiary ms over some portion of the text, forsaking his main source (compare (ii) on p.2:10), and follows that subsidiary ms at a point where it has the original reading while his main source has an error;
- (iii) he avoids reproducing an error in his main source by consulting a subsidiary ms (or the reading of the margin), which is sound at that point in the text.

Many scribes did their utmost to obtain a correct text, and so the invalidity of V was indeed one of their main objectives. Thus V is usually the most doubtful of our assumptions.

We may express the effect of the invalidity of V in terms of error sets. Suppose an error which originates in a ms M_1 and is removed by M_2 , a descendant of M_1 . Let E_1 be the set of extant descendants of M_1 , and E_2 that of M_2 . Then E_1 and E_2 are both members of \mathcal{L} . If we suppose that, apart from this action by M_2 , the assumptions of Chap. 1 have been fulfilled, then this passage will give

rise to an error set, say E' , which contains every member of E_1 which is not a member of E_2 , i.e. the difference between E_1 and E_2 . Thus the original error set is depleted by one of the error sets which it contains.

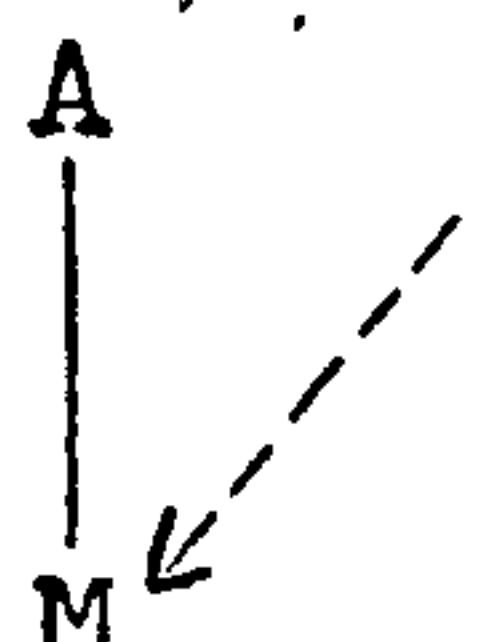

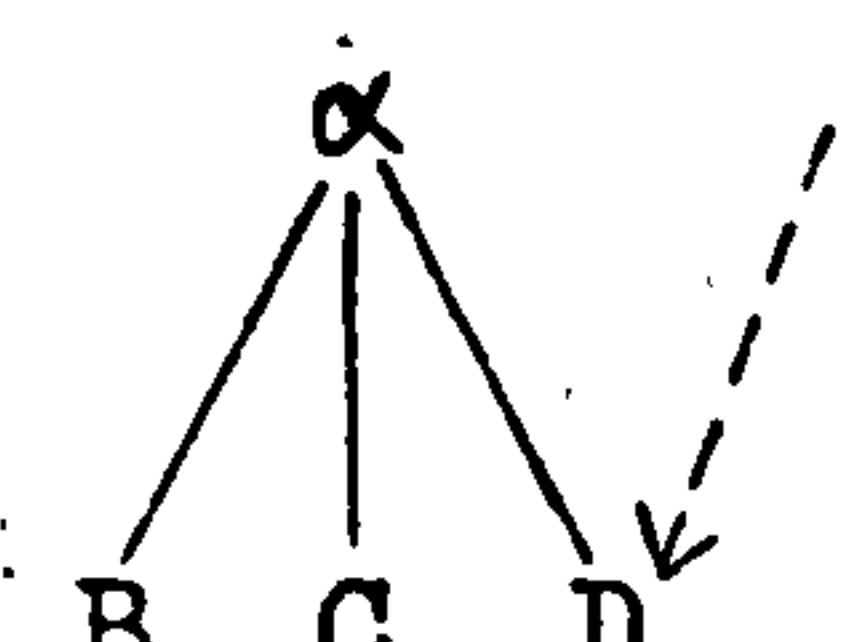
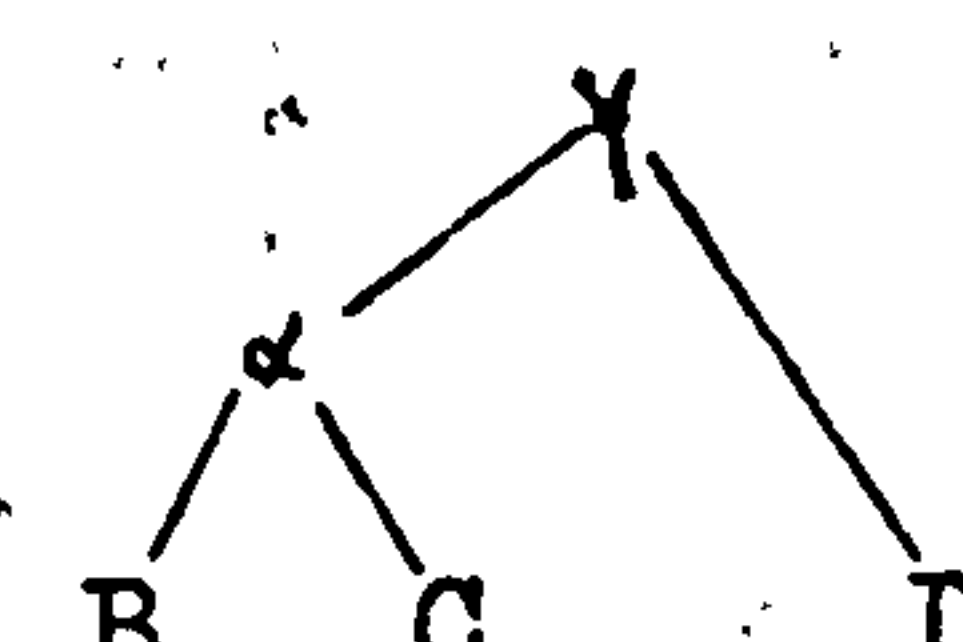
One particular case is of interest. Suppose an error in the archetype itself, which is corrected by M_2 . Then this will produce a two-way split - which there would not have been if not for that correction - where the error set consists of all the extant mss minus the mss E_2 . An example is given in connection with fig. B.2.4b.

The error set thus created gives rise to three possibilities (a) (b) (c), as in our discussion of IV; and there are three corresponding outcomes, which we now discuss.

(a) Consider once more the textual tradition of fig. B.2.2b(ii), for which \mathcal{L} comprises B, C, BC, D. Suppose that B succeeds in removing an error in γ . This will give rise to a depleted error set $C(=BC-B)$, which is itself a member of \mathcal{L} ; and by VIab, there will be other passages where C is found to constitute an error set. We shall therefore believe mistakenly that that error arose in C and does not go back as far as γ , but the construction process itself will not be affected.

(b) As in our discussion of IV, we are in danger of adding the new error set to \mathcal{L} . Thence we would postulate a "ghost" point ms from which M_1 and M_2 will appear to be independently derived; M_1 will seem not an ancestor, but but merely a collateral, of M_2 , for M_1 has an error not found

in M_2 . Fig. B.2.4 offers two illustrations of this. As before, (i) gives the true, (ii) the apparent, textual history. The symbol \dashrightarrow signifies the use of the scribe's own ingenuity, or of subsidiary sources, to remove an error in the exemplar. Fig. B.2.4b shows the operation of this process in a case considered by J. Fourquet¹ and

			
fig.B.2.4a (i)	fig.B.2.4a (ii)	fig.B.2.4b (i)	fig.B.2.4b (ii)

A. Kleinlogel². Here the "true" list \mathcal{L} consists of: B,C,D. If an error of α is corrected away by D, then we shall obtain a new error set, viz BC(=BCD-D). Hence it will seem as if BC had an xca; in other words, D will seem to be a collateral, rather than a descendant, of α .

(c) This case is similar to its counterpart in our discussion of IV. As the error sets are not all mutually consistent, we cannot yet proceed with the construction. An example is fig. B.2.5. Here \mathcal{L} comprises the error sets B,C,BC. If C removes an error inherited from A (not from β), this will create a new error set AB(=ABC-C),

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1. Romania (1948-9), p.88. Fourquet suggests that many stemmata which are in fact three-branched have thus been made to appear two-branched, and that this goes some way towards accounting for the predominance of the bifid stemma.
 2. Philologus (1968), p.78. Kleinlogel is somewhat more elaborate.

which is inconsistent with BC.

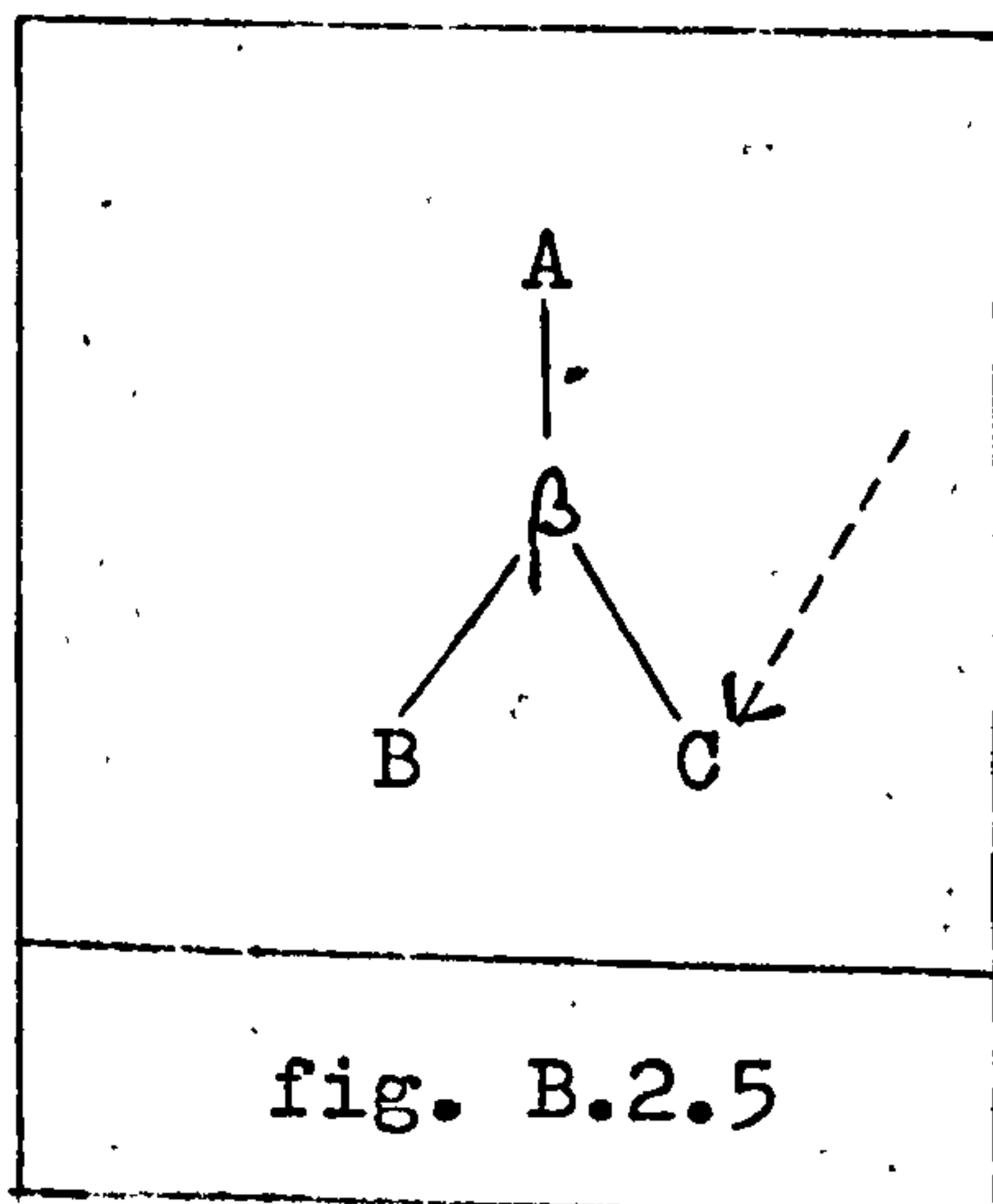


fig. B.2.5

As before, let us specify the different circumstances in which each of the three possible outcomes will be produced. Let e be the ms in which the error originated, and let d be the descendant of e in which the error was corrected away. Then e is either an arc ms or a point ms.¹ In the former case, consider the point ms at the lower end of the arc; in the latter, consider e itself. We may denote this point ms by e' . (Thus, if e is a point ms, e and e' are identical.)² Similarly we define d' . Then every extant descendant of e is also a descendant of e' , and similarly for d and d' . The two mss thus obtained, d' and e' , must be distinct; for otherwise there would be no trace of e 's error among the extant mss.

The three cases appear to be:

(a) e' is "dead", and gives rise to only two branches

1. for otherwise the extant mss would show no sign of the error (or of its correction).
 2. All this may seem an unduly complicated way of going about the problem, but I have been unable to achieve precision otherwise.

of descent; and there is no other point ms between e' and d'.

(b) There is no other point ms between e' and d'; but to exclude any case covered by (a). (Thus e' may be "alive", or may give rise to three or more branches; see fig. B.2.4).

(c) Any case not covered by (a) or (b)¹.

What means are available to distinguish the "true" error sets from the "depleted" ones? Intrinsic considerations will not suffice, for almost any error, whatever its nature, could have been corrected away by a conscientious scribe. We can only hope that the criterion of frequency will help us, but we must not be surprised to find it inadequate. For a learned copyist, especially if he has plentiful access to subsidiary sources or to marginal readings, can correct away a great number of errors in his exemplar, and thus cause a "depleted" error set to be attested with a frequency to rival that of the "true" error sets. Thus the dangers of (b) and (c) are very real.

Let us now consider these two dangers in somewhat greater detail. Possibility (b) implies that the stemma which we infer, even if it accounts for the textual data perfectly well, may give an utterly false textual history. Suppose, for example, that we have extant three mss ABC, and that we find in the construction domain many occurrences of the following error sets, and no others: A, AB, B, C. Then we shall deduce the stemma of fig. B.2.6a. But the

1. Here too (b) and (c) are mentioned by Froger (p. 106), according to whom (b) occurs only when a ms fails to reproduce "une faute commise par son ancêtre immédiat" whereas (c) is found "lorsqu'un manuscrit est privé d'une faute commise par un ancêtre lointain" (my italics). But once again, the closeness of the ancestor is not in itself the deciding factor.

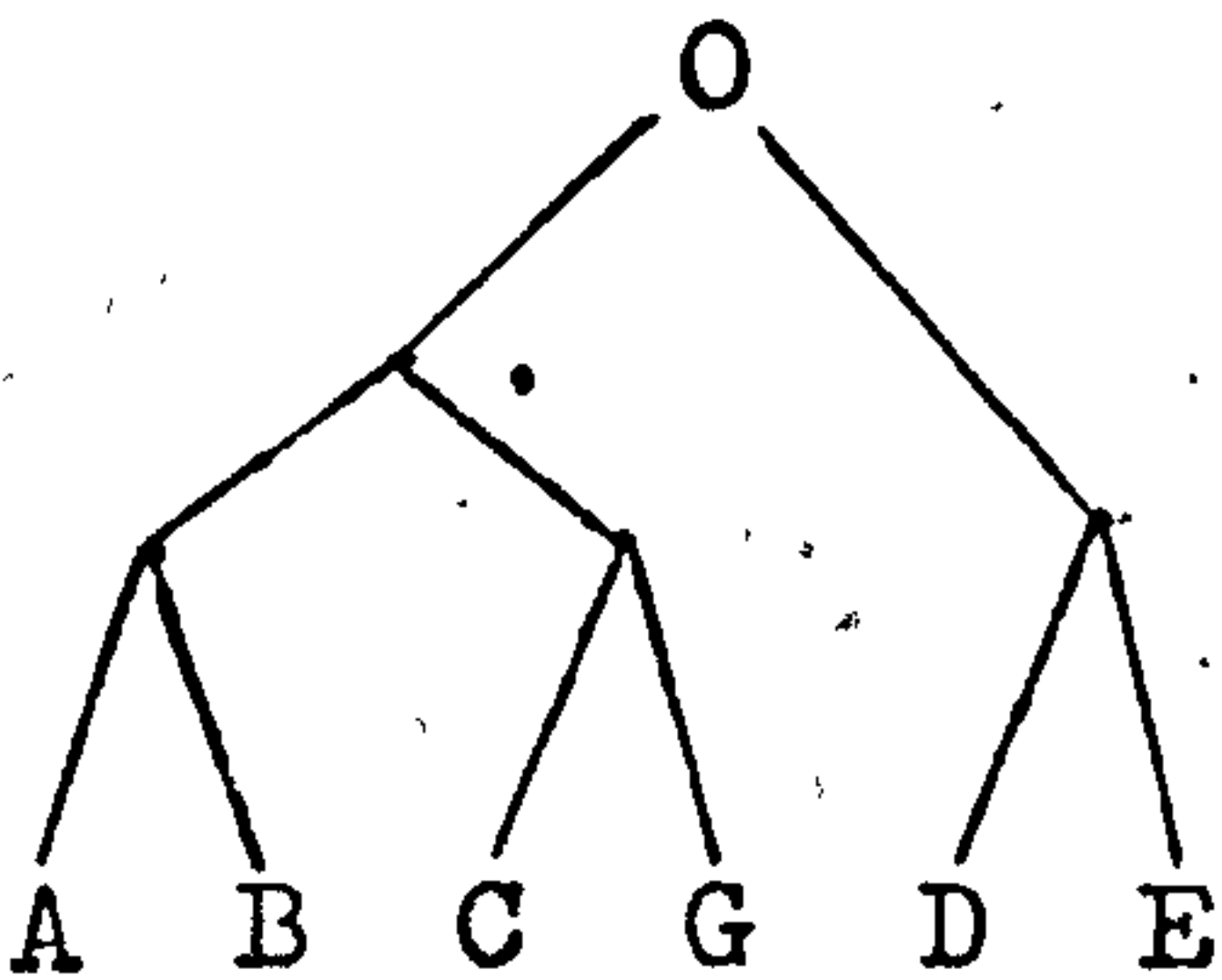
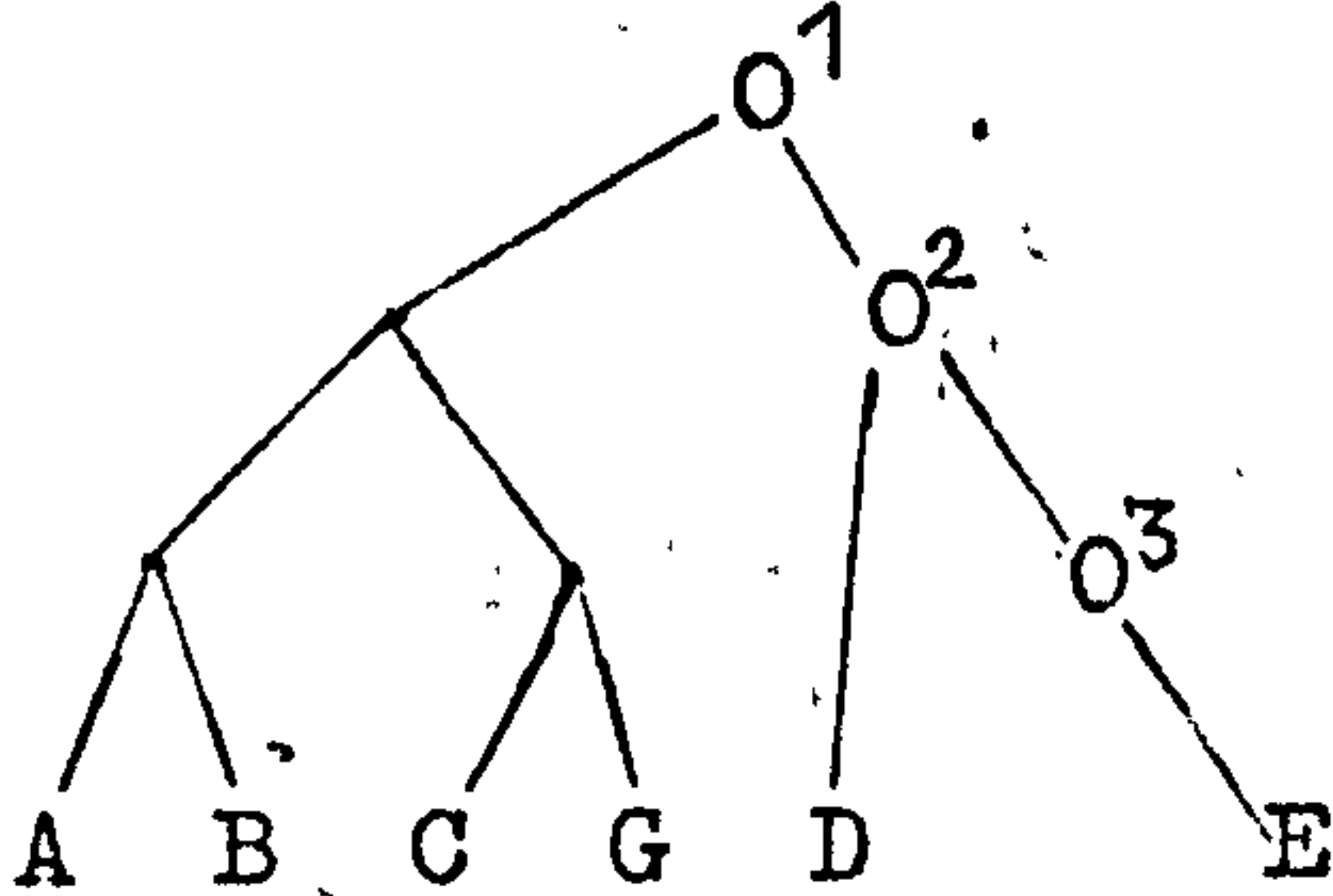
figure shows other possibilities. Perhaps ABC are all

fig.B.2.6a	fig.B.2.6b	fig.B.2.6c	fig.B.2.6d.

direct copies of ω , and C is the work of a scribe who utilised another source to correct away sundry errors of ω ; in that case AB would be not a true, but a depleted, error set ($AB=ABC-B$). Again, C itself may be the archetype, from which x was copied by an editor who recovered several correct readings lost in C, though he added some errors of his own. Or another possibility (d): readings of A were inserted in the margin of y, and were taken over on some occasions by B, which is nevertheless derived primarily by an independent line $\omega-x-y-B$ from ω . Yet in the absence of any new evidence, we shall not suspect that (a) does not give a true picture. Now the rules which we made in order to identify the original reading in the application domain were based on the assumption that our stemma agrees with the facts. Thus if AC agree against B, we shall prefer the reading of AC as original, even if we do not make all the assumptions of Chap. 1; for we shall think it more likely that B's reading is an error committed in copying α , than that AC independently originated the same error. But all such arguments fall to the ground when we perceive that the textual history may have

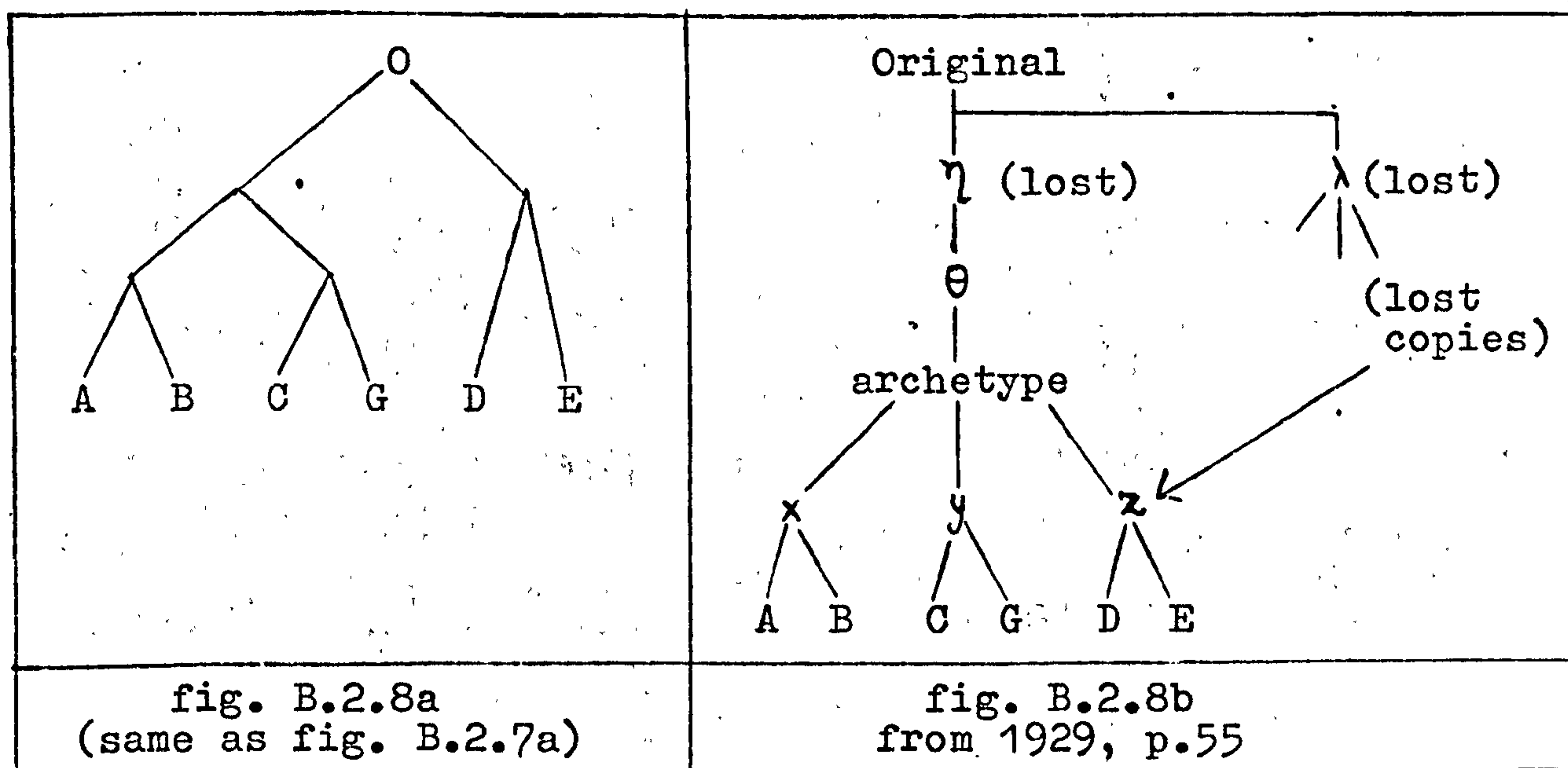
been quite different from what is suggested by the stemma.

This charge, that the textual history suggested by the stemma may be untrue, was first laid by J. Bédier. In an edition of the old French text "Le Lai de l'Ombre" by J. Renart¹, and later in an article in the journal *Romania*², he claimed that the textual data was compatible with at least eleven different textual histories, each of which had different implications for the critic who hoped to establish his text thereby. Of these alternative histories, some³ involve the supposition of a multiple original (cf. I), as in fig. B.2.7b; these will not disturb unduly the scholar who is studying a

	
<p>fig. B.2.7a; from 1913, p. xxv (=1929, p. 6)</p>	<p>fig. B.2.7b; from 1929, p. 67</p>
<p><u>Note</u> A seventh ms F, the affiliations of which are particularly difficult to recognise, has been left out of both figures.</p>	

1. "Le Lai de l'Ombre, par Jean Renart, publié par Joseph Bédier", Paris 1913; a volume in the collection published by the Société des Anciens Textes Français.
2. "La tradition manuscrite du Lai de l'Ombre: réflexions sur l'art d'éditer les anciens textes", in *Romania* (1928), pp. 161-196, 321-356. This was re-published in Paris in 1929, and my references are taken from this monograph. Bédier's two studies are cited as 1913 and 1929.
3. See 1913, p. xxxvii; and 1929, pp. 66f.

tradition which affords little reason for supposing that there was more than one original¹. Some other alternative histories suggested by Bédier imply the invalidity of VIc (and occasionally VIb); but here again there are other fields in which our knowledge of the author's usage and so on is good enough for us to be rather more confident of VIc, and hence to attach less weight to Bédier's objections². Only one of his alternative stemmata, as far as I can tell, rests solely on the invalidity of III (and hence V); it faces us, essentially, with the dilemma between possibilities (a) and (b) in fig. B.2.6. That stemma is shown in Fig. B.2.8b :



1. The presumption must be initially in favour of a unique original (cf p. 7:11). So Pasquali (p. 419): "le <<varianti d'autore>> sono l'ultima ratio della critica testuale, e non è lecito ricorrere a esse, finchè le divergenze si possano spiegare in qualsiasi altro modo".
2. See further our discussion below, on VI.

If it were agreed that the complete list of error sets was: A, AB, ABCG, B, C, CG, D, DE, E, G; we could not tell whether the true history was that given in fig. B.2.8a, or whether AB, CG, DE derive from three independent copies x, y, z of the archetype, z having corrected away some of the errors of that archetype by recourse to mss from a now lost branch. Now Bédier would argue that as long as there were two (or more) possible explanations, we could not be justified in simply adopting one particular hypothesis and choosing our readings in accordance with it. "Assurément, tout se passe comme si le classement de dom Quentin [one of the eleven alternative histories] était le vrai; mais, pareillement, tout se passe comme si cet autre était le vrai, et cet autre encore, et ainsi jusqu' à onze. Or la formule Tout se passe comme si... convient dans les sciences purement spéculatives; elle n'autorise pas un médecin à offrir à son malade une potion dont il saurait qu'elle risque de le tuer; elle n'autorise pas un constructeur de schémas [=stemmata] à traiter son texte d'après un canon critique dont il saurait qu'il risque de l'abîmer" (1929, pp. 69f). Thus Bédier would not allow any distributional inference from any stemma.

The reader can easily verify for himself that, given virtually any stemma, we may generate a variety of alternative textual histories. Let us leave out of account those alternatives which involve the invalidity of I and VI, postulates which may enjoy far more justification within traditions other than those with which Bédier dealt. But wherever we recognise that the invalidity of III (and hence IV and V) is

a real possibility - which it virtually always is - any stemma we may draw will be liable to Bédier's charge, which has to be answered before we can make critical use of the same.

Thus the scholar who is aware of Bédier's arguments and nevertheless contemplates making such use of the stemma, must subscribe to what may be termed an axiom of equivalence, on the following lines:

Suppose that assumptions (I) and (VI) hold good, and that the evidence (both objective and subjective, in the sense of p.1:15) leads us to a stemma which represents a history H_1 . Then the true textual history may be quite different¹ from H_1 ; we may denote it H_2 . Nevertheless, any inference which is valid under the assumption that H_1 is true, will be equally valid if the textual history is in fact H_2 . In other words, two textual histories which produce the same textual data in the construction domain, can be treated as equivalent for the purpose of distributional inference.

Bédier would argue that there was no justification for this proposed axiom, and that all stemmatic inference was

1. Of course H_1 itself properly denotes not one textual history but a whole class, in that the numbers of arc mss and traceless mss are not specified. But H_2 does not fall within that class, for it differs from H_1 with respect to the point mss.

therefore fallacious. Yet I feel that there is something to be said in favour of it¹, both on empirical and on theoretical grounds.

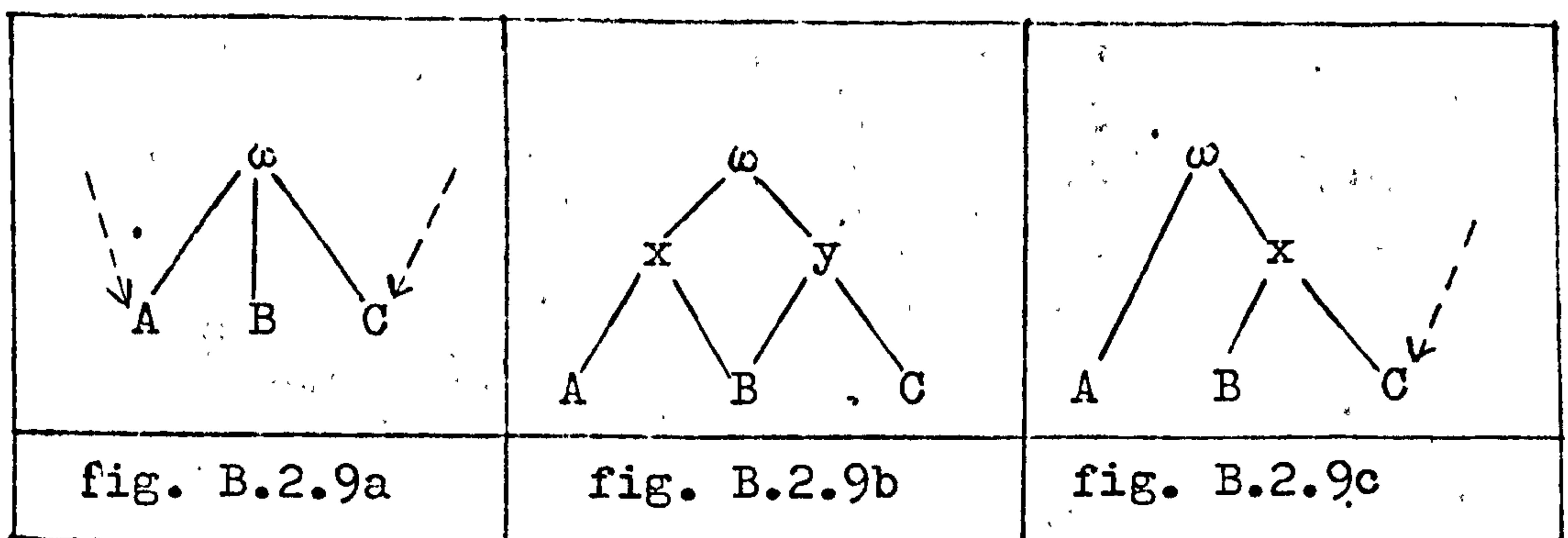
Let us take the empirical evidence first. Fig. B.2.6a would admit the following distributional inferences:

If the mss split A:BC or B:AC, we prefer the reading of C. If they split AB:C, we have no distributional grounds for preferring either of the rival readings to the other.

If the reader will consider in turn each of the textual histories bcd of fig. B.2.6, he will find these rules applicable. Let us take (d). When the mss split A:BC, A's reading is likely to be an error made in transcribing ω ; similarly for B:AC. But when we have AB:C, then either C's reading is an error in the same way, or B has adopted an incorrect A reading from the margin of y and thus C alone has the true reading. Thus, if the true history is given not by (a) but by (b), (c) or (d), we shall not go far wrong in our choice of readings if we behave as if ~~the~~ textual history were in fact (a).

-
1. It should be made clear that by this axiom it is not suggested that, of all the alternative histories, we should presume that which takes the form of a stemma (without contamination) to be correct, perhaps on the grounds that it is the simplest. Presumption in favour of the simplest of a choice of hypotheses was elevated into a principle by Hill, who advocates a "postulate of simplicity". But as Dawe has said in another connection, economy is not the same as truth. If such a presumption is our only authority for adopting the stemma and basing thereon our choice of readings, we are indeed on dangerous ground.

I have considered a number of hypothetical cases, but have not found any counter-example against the axiom, which indeed seems to apply even when none of the alternative histories takes the form of a stemma. Suppose, for example, that we have three mss ABC, and can identify several passages where each has an incorrect reading while the others are sound, several other passages where AB agree in error against C, and yet others where BC agree in error against A. Thus there is solid attestation for each of the error sets A,B,C,AB,BC. Each of the histories of fig. B.2.9 is compatible with the facts¹:



Let us suppose that possibility (a) is the true one. We may set up distributional inferences as follows: If AC agree against B, then it is likely that B's reading is an error, and that the shared reading of AC is original. But if AB agree against C, then perhaps C's reading is an error, or perhaps ω had an error which C corrected away, and thus

1. Again the arrow \dashrightarrow denotes correction, by recourse to a subsidiary ms. In (b), B is a conflation of x and y.

C alone preserves the truth; so no distributional inference is possible if the mss split AB:C or (similarly) BC:A. Finally, if ABC are all different, then we cannot tell which reading is original. In short, the only situation where we may be reasonably confident is where AC agree. Now if we had instead assumed (b), our conclusion would have been the same. A reading shared by AB may go back to an error in x; one shared by BC to an error in y; but a reading common to AC is probably that of ω , which is the closest we can get to the original without resorting to emendation. And if we had in turn supposed that (c) gave the true history, then the same inferences would apply. Thus the axiom seems to be true in those theoretical cases where I have tested it.

There does seem to be some theoretical justification for the axiom, at least in the case where one of the possible histories yields a stemma. For when we make distributional inferences, we reason as follows. Two mss which have no common ancestor later than the archetype - assuming that our stemma is true - agree in a certain reading. This may be due to coincidence, or to occasional borrowing; but it is rather more likely that they both preserve the reading of the archetype. Now our statement that the two mss had - or rather appear to have had - no ancestor in common besides the archetype, is itself based on the fact that in none of the error sets on which the construction of the stemma is based do those two mss appear together. That is, they do not agree in error throughout the construction domain. Now this is a fact (as long as our judgments on correct and incorrect readings are accepted), no matter how doubtful we may be about the true

explanation of that fact. Perhaps their respective source-complexes had little in common, apart from those sources which are common to all the extant mss; if that is so, then whenever they are found to agree, there is a presumption that they preserve the reading of a common source, which is on that hypothesis comparatively remote and may well have carried the original reading. Or perhaps there were many corrupt mss common to the two source-complexes, but the efforts of scholarly editors, in one of the two lines of descent or in both, ensured that only rarely were corrupt readings left uncorrected, to survive in both those extant mss; so in that case too, the agreement of these two mss is likely to take us back to a relatively ancient common source¹.

Although these arguments seem more or less sound, they fall far short of the rigorous logic of Chap. 1. Yet once the possibility of contamination is admitted - even though it has been found possible to draw a stemma - we can no longer apply the stemma by deducing successively the readings of the

1. This is based on Bévenot's theory of "disconnexions". Bévenot employs it, however, not as a rationale for the application of a stemma, but as an approach to the study of highly contaminated traditions, such as that of Cyprian. In his paper of 1970 (see Thes., p. 11:113), he says, of two mss (m and Y): "I suddenly recognised that the important thing for me was not the frequency with which they thus agreed [sc. in departing from the original reading], but its infrequency. Consider these two mss; between them they depart from the [supposed correct] text (with others) 103 times, but these two agree in doing so together only three times. It is a fair conclusion that they were not copying the same manuscript, nor was there much or any borrowing between their respective lines of ancestors. Such a pair of manuscripts I call 'opposed' mss, their independence of each other favouring those readings where they do agree". On his development of this theory, see his book of 1961, pp. 133ff., 148ff.

lost point mss until we arrive at the reading of the archetype itself, for the true textual history may differ entirely from what was assumed in that reconstruction. The alternative rationale offered above - involving the axiom of equivalence - does at least attempt to come to terms with the realities of textual transmission. It acknowledges that the stemma gives only a textual history which is compatible with the data, not the textual history; and it holds out the hope that the critical inferences to be made from the stemma will merit our confidence more than the stemma itself.

What has been suggested above may be formulated in terms of a distinction which has sometimes been made between bibliographical and textual relationships between mss, in the following terms. If we put A below B in a stemma, this could be taken to mean either that A is in fact derived from B, directly or through an intermediate copy, or that the text of A is derivable from that of B, i.e. this possibility is admitted by the evidence although we acknowledge that the true history may be quite different. The former statement may be termed bibliographical, the latter textual¹. It would at first seem that only the former type of statement would be capable of yielding any valuable conclusion concerning the choice between rival readings; as Greg puts it, "if we wish to arrive at really reliable results we ought always to

1. Particular emphasis is laid on this distinction by V. A. Dearing, in his Manual (see Thes., p.11:34), p.3.

be sure that our textual conclusions, by whatever road we have reached them, are capable of proof on rigidly bibliographical lines".¹ Now Bédier has shewn that a stemma cannot hope to be more than a textual diagram, in that the bibliographical relations are indeed doubtful; thus he deduced that nothing of value can be inferred from any stemma. The axiom of equivalence, however, suggests that a diagram of the textual relationships, as long as it is compatible with the data, can be used as if it gave the true history.

We may remark that some writers seem to have taken for granted the axiom of equivalence, without stating explicitly either the axiom itself or any justification for it. Thus Hill envisaged drawing stemmata which were not valid bibliographically, and using them to reconstruct the text of the archetype: "At the end of all his labors, the editor presents his results in the shape of a family tree, leading back to 0 [the archetype], whose reconstructed² readings he will follow in his text. I should repeat that a tree is a description of the relationship of readings found in mss, and ought never to be understood as a statement that A was copied from B. It merely states that the readings now found in A are derivable from readings now found in B, after examination of all the extant evidence... Actually an editor who has

1. "Bibliography - An Apologia", in Library xiii (1932), pp. 113-143; see p. 124.

2. my italics.

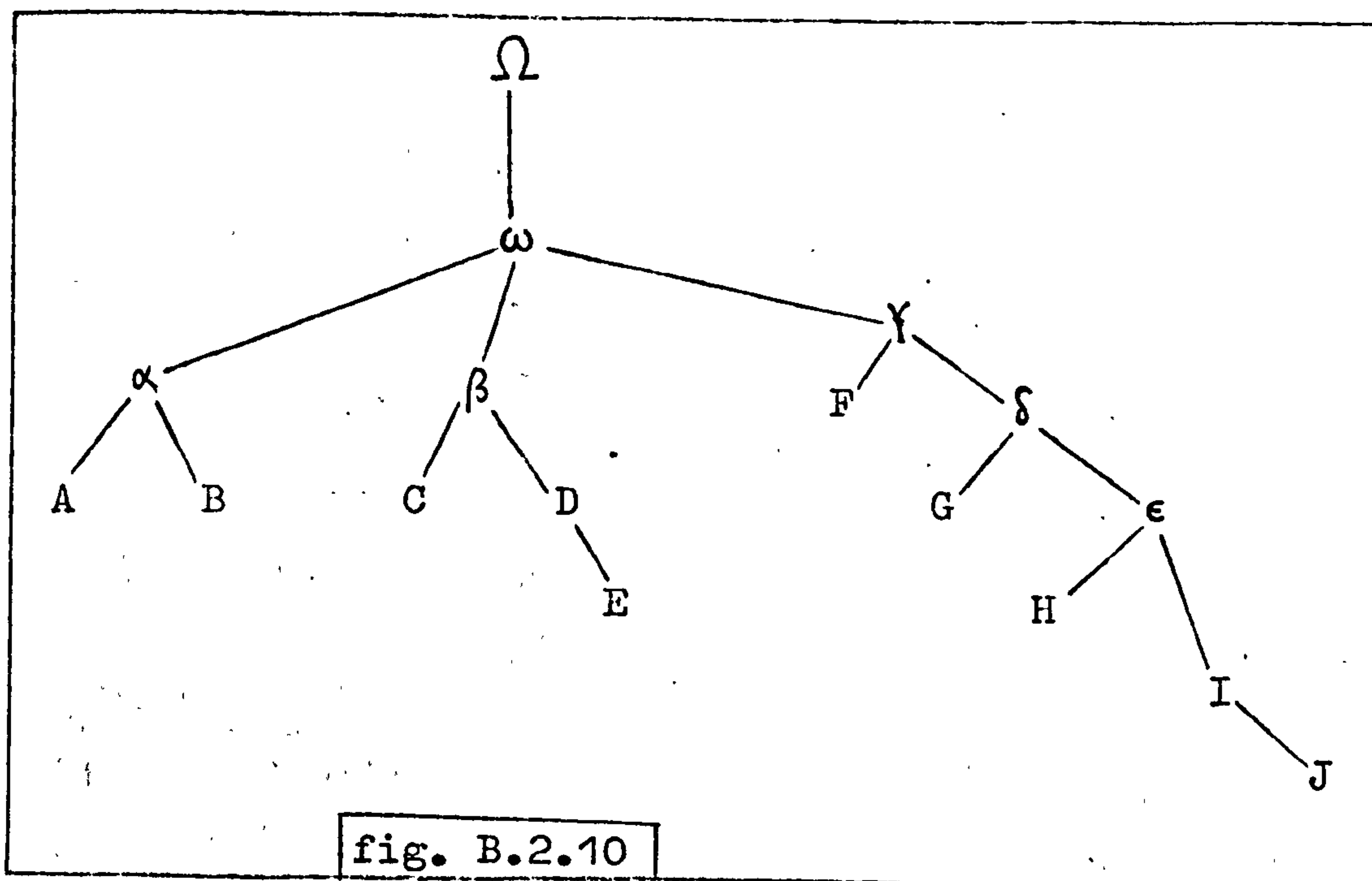
derived A from B should be quite unmoved by external evidence that B is on twentieth century paper, while A is on mediaeval vellum" (pp. 84f.). But if the tree does not necessarily reflect the historical facts, then Hill would not be justified in proceeding without further ado to "reconstruct" the readings of O; for the methods whereby we seek to recover successively the readings of the ancestors of the extant mss rest on the assumption that the textual history proposed by us is correct, and that the scribes involved may be expected to have behaved in a particular way¹. Another instance of an implicit appeal to the axiom may be found in Castellani's book already cited². Let us suppose three families of mss, independently derived from an archetype; and let there be contamination between two of these families. Then we shall find several places where they agree in error against the third family, and we may mistakenly suppose they form only one family. (This is essentially the case of fig. B.2.6d, which may appear as (a).) Then this mistake on our part will not be as serious as we might have thought. "Il faut noter...qu'un schéma où deux familles contaminées n'en forment qu'une fonctionne aussi bien, au point de vue de la constitution du texte, que le schéma génétiquement exact où les deux familles remonteraient à l'original" (p.37). But neither Castellani nor any other

1. e.g. that the changes made in two different transcriptions of the same exemplar only rarely coincide, and hence it is likely that wherever two such copies agree they reproduce the reading of that common ancestor.

2. See p. A:68.

writer known to me gave a proper rationale for making critical inferences from stemmata despite the suspicion cast upon them by Bédier.

We now come to discuss possibility (c), that we shall not find enough mutually consistent groupings to be able to draw a satisfactory stemma. This is indeed likely in a tradition wherein scribes have made good use of collations to improve on their main exemplars. Each error which is detected and removed, and hence prevented from appearing in one or more of the extant mss, will give rise to a "depleted" error set; and according as the pattern of success achieved by scribes varies from one passage to another, many different "depleted" error sets will be produced, and there is no guarantee that conditions of mutual consistency (Lemma 6) will be satisfied by those error sets and hence by the variant sets which we obtain when we first try to compile a construction domain. Consider for example the stemma of fig. B.2.10.



Here we have ten mss A...J, all descended from an archetype ω which is in many places corrupt. The efforts of scholars to remove the inherited errors of ω may result in many different error sets in different passages, e.g.

ABCDEF	(through correction by δ)
ABFG	(" " " β and ϵ)
CFGHIJ	(" " " α and D)
ABFGHI	(" " " β and J)

Each of these four is inconsistent with two of the other three. Similarly the errors of γ , say, will often result not in the "true" corresponding error set (FGHIJ) but in various depleted forms thereof, such as FG (through correction by ϵ), GIJ (correction by F and H), FHI (correction by G and J), and so on; the scribes may indeed have been so perceptive that the full error set is found no more often than are some of its depleted forms, and will not therefore reveal itself as "true" by the criterion of frequency.

We may expect to find an inverse association between the size of a true error set and the likelihood that it will be attested frequently enough to be acknowledged as constant. Thus G. Kane, who attempted to apply Greg's calculus to the mss of Piers Plowman, writes (p. 98): "One is prepared for the agreements supporting larger groups to be fewer than those supporting, say, genetic pairs. The remoteness of the act of copying their exclusive common ancestor establishes a probability that further variation will have obscured many

shown (in black). It is remarkable how steeply the size of the tolerance set rises as we proceed towards the top of the stemma. Thus the true error set {31, 47, 50, 52, 54, 64, 75} will appear only if an error committed by the xca (or its immediate ancestor) is tolerated by twelve (or thirteen) copyists, whereas its constituent error sets, such as {54, 75}, will require the tolerance of not more than four copyists. If we were to assume as a gross simplification that every copying introduced 20 errors, and every error had a 20% chance of being corrected away by any given copyist, then we would expect to find about 8 occurrences of the error set {54, 75}, 16 of the error set {31, 50} and 18 of the error set {47, 52, 64}; but only two or three¹ occurrences may be expected of the 7-member error set {31, 47, 50, 52, 54, 64, 75} in which they are all contained.

There is another (perhaps less powerful) factor which tends to make a large error set unstable. The greater the diffusion of a reading, the greater the likelihood that it will be borrowed by other branches of the tradition, and that the error set will be disfigured by augmentation.

We may conclude, then, that in a tradition wherein scribes frequently compared exemplars, there cannot have been many erroneous readings which enjoyed widespread diffusion, were acceptable enough to be tolerated by a considerable number (say 20 or more) of copyists, and were at the same time not attractive enough to be adopted by anyone else; yet nothing less is required in order that a true error set of, say, six or more extant mss be realised in practice.

The best way of assessing danger (c) is not to theorise further but to examine actual traditions. Let us first consider the Cyprian data. Professor Bévenot² regularly reports eighteen

1. actually about $2.5 \left(\div 20 (0.8)^{12} + 20 (0.8)^{13} \right)$.

2. "The Tradition of Manuscripts: a study in the transmission of St. Cyprian's Treatises", Oxford 1961.

mss, whose sigla are: a b B D e G h H J k m O p P R T W Y, over the treatise De Unitate. From his apparatus I extracted all the variants, except for trivialities of spelling, for some special cases mentioned below¹, and, of course, for unintentional omissions on my part. There were in all 444 places² where the mss diverged, and, of these, 346 yielded "perfect" two-way splits, in which the readings of all eighteen mss were available and each ms presented one or other of two readings. When I had obtained the 346 corresponding variant sets, I found that they represented no less than 198 different types, many of which were mutually inconsistent. Thus it was necessary to discriminate between them on grounds of frequency. In Table B.2.2 are given the more frequent variant sets; the "base" ms (denoted A on p.1:31) was taken to be e.

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1. The study of the De Unitate was undertaken with the object of testing not only the stemmatic method but also the "mapping" technique developed in this thesis. I therefore omitted all variants in Chapter 4 of the treatise, which is extant in two considerably different forms, going back (as Bévenot discovered) to a first and to a revised edition respectively, both from the hand of Cyprian himself. This omission of Chap. 4 on my part enabled me to use the behaviour of the mss in that Chapter as an independent check on my results. Cases in which only one ms diverges from the rest were also ignored; for the purposes of this discussion, we need remark only that any variant set due to that cause will be consistent with any other variant set whatsoever.
 2. As there has been a certain subjective element in the selection of the variants, and I may have moreover been guilty of some inaccuracies, I doubt whether other students would agree exactly with these numbers, but I doubt equally whether such disagreement would affect our conclusions.

TABLE B.2.2.

Variant set	Number of occurrences
mP	53
HT	20
hHT	11
kP	8
WY	7
aG	6
bO	5
DR	5
aJ	4
bR	4
BR	4
DH	4

The noise-level lies between 4 and 5, in that the splits are consistent until we come to aJ, which conflicts with aG, and to bR, which conflicts with bO, and so on. Thus the only splits which can claim to be constant are the first eight, which cover altogether 115 passages out of the 346. Moreover, the border-line which marks off the constant groupings is very fine; it is hardly satisfactory to group together b and O, on the grounds that the variant set bO is found five times while the set bR occurs only four times. If we take these 115 passages as a construction domain, then we have grounds for complaint on both of the counts set out at the beginning of this chapter.

Moreover, our fears of a dearth of constant groups containing a larger number of mss, whereby the mss might have been divided up more evenly, have been confirmed. The largest group which we have been able to establish contains only three mss. The effects of the tendency of larger groupings to repeat themselves only seldom - and in most cases not at

all - is illustrated in Table B.2.3., in which the 198 different types of two-way split are broken down according to frequency and "size" (i.e. sixteen mss against two, fifteen against three, etc.).

TABLE B.2.3

Size:		<u>2+16</u>	<u>3+15</u>	<u>4+14</u>	<u>5+13</u>	<u>6+12</u>	<u>7+11</u>	<u>8+10</u>	<u>9+9</u>	Total
<u>Frequency</u>										
1		33	50	21	22	10	13	10	5	164
2		11	2	1	1					15
3		4	3							7
4		4								4
5		2								2
6		1								1
7		1								1
8		1								1
11			1							1
20		1								1
53		1								1
Total		59	56	22	23	20	13	10	5	198
Mean frequency		3.18	1.32	1.04	1.04	1.00	1.00	1.00	1.00	1.75

Note: The groupings entered below the double line are constant.¹

Although this table covers only 198 elements, we cannot mistake the concentration of entries towards the top left-hand corner. More fully,

- (1) groupings that divide the mss into two sets of very dissimilar size predominate;

1. to use Greg's term; that is, their frequency exceeds the "noise-level" (p. 1:34).

- (2) most of the 198 split types occur only rarely, and there are very few which are frequently attested¹;
- (3) the mean frequency decreases as the "size" increases, and all splits whose "size" exceeds a certain limit (in this case 5) occur only once.

It is the third of these tendencies that has frustrated our attempt to reconstruct a stemma. On the basis of our constant variant sets, we might tentatively have drawn a few links in the tree:

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- 1. The distribution seen in the last column of the table bears a marked resemblance to the word frequency distribution, first investigated by G. U. Yule, "The Statistical Study of Literary Vocabulary", Cambridge 1944; thus a high proportion of an author's vocabulary will consist of words which he uses only once in the work under analysis, a smaller proportion will consist of words used only twice, and so on. The similarity is not unexpected; in either case there are a great number of "types" which could potentially occur (vocabulary items; two-way-split types, numbering $2^n - 1$ for n extant mss, i.e. over 130000 for $n = 18$ as here) but vary considerably in likelihood of occurrence (some words being required far more often than others; some splits being due to straightforward, others to more complex, processes of transmitting readings). Perhaps Yule's Characteristic K could be used as an (inverse) index of extent of contamination?

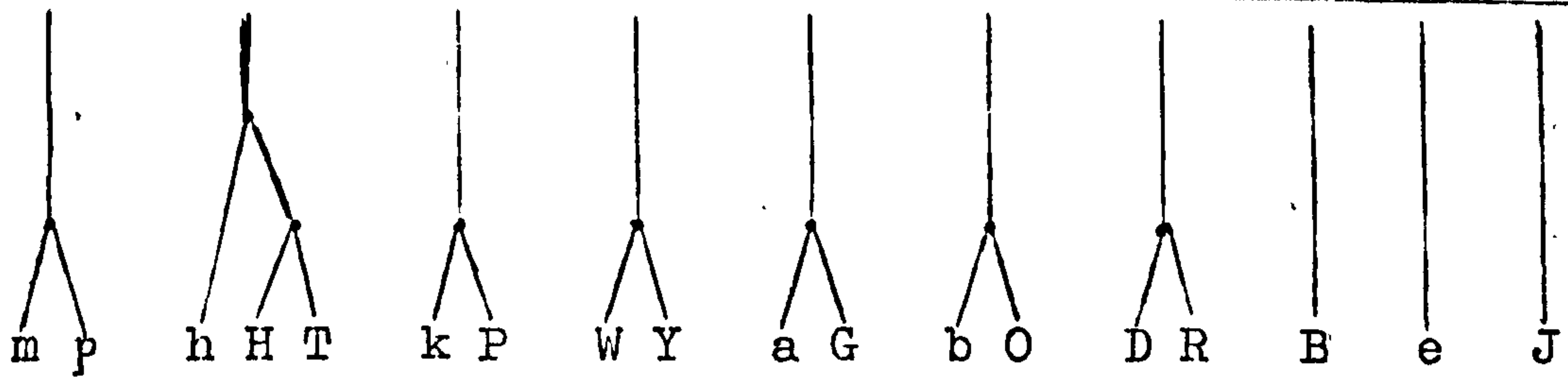


Fig. B. 2. 12

but the absence of other constant groupings leaves us with no means of relating these small groups to one another. We are reminded of Bédier's verdict¹ on the majority of stemmata constructed by scholars: "la base de la construction, le rez-de-chaussée, est solide. Mais il en va autrement des parties hautes".

Let us now turn to the Persae. Here I used the collations of R. D. Dawe², which report fully the 16 mss: A B C Δ H I K M N Nd O P Q V Y Ya. I worked on lines 1-746 only, after which N is not available; see Dawe, pp. 297-330. The attitude I adopted towards variants was perhaps inhospitable³, but did satisfy me that the variants admitted

1. 1929, p. 71. But Bédier, as we have seen, was making a rather different point, with regard to cases wherein there were too many possible stemmata.
2. "The Collation and Investigation of Manuscripts of Aeschylus", Cambridge 1964.
3. I excluded variants consisting only in word divisions, breathings, accents, regular elisions, epheletic nu, iota subscript (or adscript) or dialect forms (e.g. $\nu\alpha\omega\nu / \nu\eta\omega\nu$, l. 39). I demanded moreover that the alternative readings be phonetically distinct according to the pronunciation of Byzantine scribes (as far as we can reconstruct it), even though they may differ in sense. Thus I discarded variants such as $\lambda\alpha\beta\epsilon\omega / \lambda\alpha\upsilon\epsilon\omega$ (l. 110) and in particular those which lay only in the oppositions $[\epsilon, \alpha]$, $[\eta, \epsilon]$, $[\upsilon, \epsilon]$, $[\sigma, \alpha]$, $[\upsilon, \alpha]$ or (if we may use Modern Greek pronunciation as a guide) in the doubling of consonants (e.g. $\lambda\eta\mu\alpha\tau\iota / \lambda\eta\mu\mu\alpha\tau\iota$, l. 55).

to consideration consisted in "real" differences. Thus I compiled a collection of 404 places where the mss diverged; of these, 242 were two-way splits where the readings of all 16 mss were available. The number of different types of two-way split observed among those 242 was found to be 143. If we take A as "base" ms, and list the 143 variant sets in order of frequency, our list will begin:

Table B. 2. 4

Variant set	Frequency
OY	19
KQ	14
BΔH	9
NNdV	8
BCΔH	7
NNdPV	7
CH	6
ΔH	6

Here the noise-level is between 6 and 7, for the set CH is inconsistent with BΔH. This yields only five constant variant sets, which altogether account for 64 of the two-way split passages, and leave unexplained 178. As in the case of Cyprian, this construction domain is unsatisfactory on both grounds (see p..2:3), and only parts of the stemma (mainly at the base) can be established:

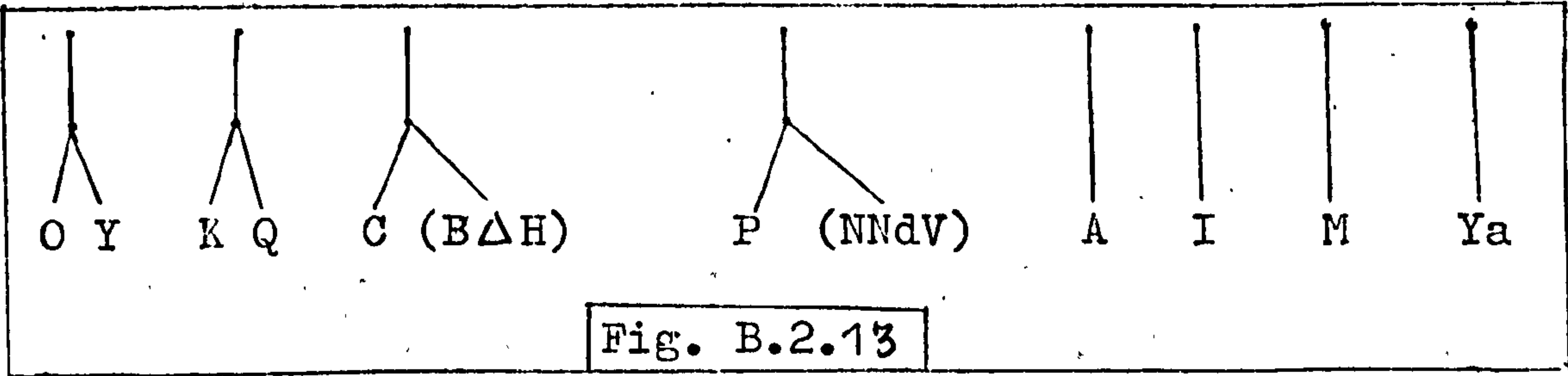


Fig. B.2.13 shows eight "twigs" of the tree, which cannot be related by this method to one another. Moreover, even within the groups BC Δ H, NN δ PV, the internal relationships are not given; we did not find this in the case of Cyprian. But the general picture is as before, with no recognisable pattern in the "larger" splits. In Table B.2.5, the 143 different two-way-split types are broken down according to "size" and frequency of occurrence within the domain studied. It is analogous to Table B.2.3.

TABLE B.2.5

Size: <u>2+14</u> <u>3+13</u> <u>4+12</u> <u>5+11</u> <u>6+10</u> <u>7+9</u> <u>8+8</u>								Total
<u>Frequency</u>								
1	23	29	27	15	9	20	4	117
2	5	2		3				10
3	2	1	1					4
4	3							3
5	1							1
6	2							2
<hr/>								
7			2					2
8		1						1
9		1						1
14	1							1
19	1							1
<hr/>								
Total	38	34	20	18	9	20	4	143
<hr/>								
Mean frequency	2.66	1.56	1.70	1.17	1.00	1.00	1.00	1.69

All the three conclusions to which our Cyprian tables led us are valid here too. The stemmatic approach has allowed us to identify a few small groups but no large ones (of, say, five or more mss).

My trespasses into the field of Middle English reveal a similar position. In their study of the text of Chaucer, J. M. Manly and E. Rickert¹ attempted to apply Greg's method to a collection of some 80 mss. Their search for constant groups revealed a number of pairs but it seems that no constant group could be found containing more than six mss (vol. ii, pp. 49 ff.). Again, let us consider Kane's study of the A version of Piers Plowman. In his introduction, Kane lists (pp. 69-96) all ms groupings which are at all persistent (i.e. which occur about ten or more times), seventeen mss being considered. Listing the groups in order of persistence, we obtain:

V	H	some 230 times
R	U	some 210 times
W	N	some 130 times
T	Ch	H ² some 90 times
E	M	some 80 times
M	H ³	nearly 80 times ²

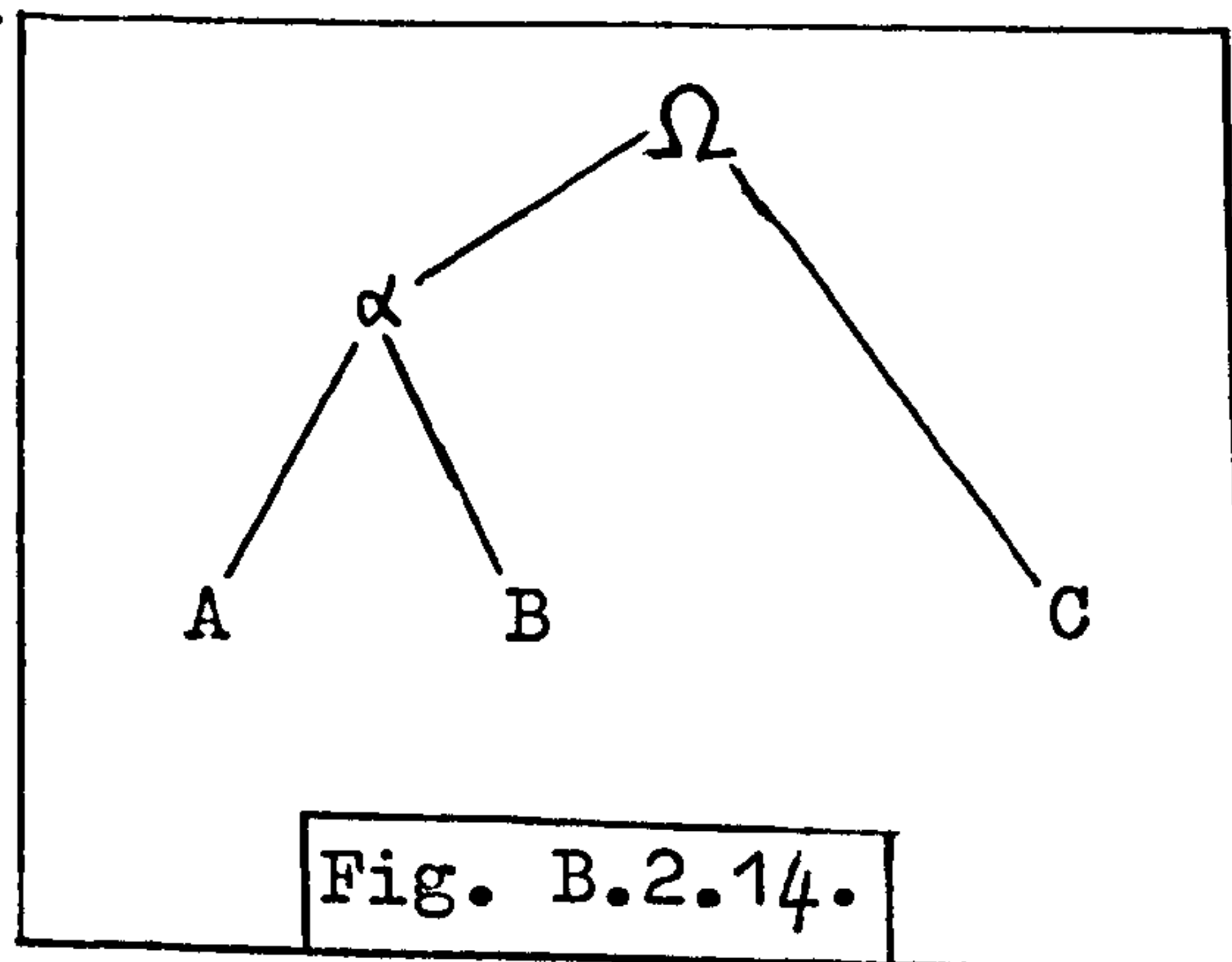
Thus the noise-level is around 80, for EM conflicts with MH³; and once again the construction domain does not take us very far. Kane's own verdict admirably sums up the

1. "The Text of the Canterbury Tales", Chicago 1940.
2. Kane lists nearly 80 instances, but heads that list: "The manuscript H³ in some 60 agreements with M".

situation: "The conclusions are firm, and seem to me beyond doubt: first, that convergent variation (whether correction or coincident error) has so obscured the evidence by which common descent might be ascertained that no clear or unquestioned genealogical picture takes shape; second, that any 'family tree' of these manuscripts, or any description of their agreements in variation less detailed than a line-by-line account is grossly oversimplified and useless for purposes of recension; and last that while some genetic groups....can be detected, there is no clear evidence to indicate their ultimate relationship and thus to form a stemma, previous 'demonstrations' of such relationship having been achieved entirely by petitio principii" (p.55). These conclusions seem to apply not only to the mss studied by Kane, but also to those of the Cyprian and Aeschylus texts. The stemmatic method in practice will virtually always yield many smaller groups; but it is the absence of regular large groups that has disappointed the editors of a great variety of texts. There is only one statement in the above extract from Kane which I would hesitate to apply to contaminated traditions in general, viz that any description of the behaviour of the mss "less detailed than a line-by-line account is...useless for purposes of recension". As a line-by-line account of the textual history is beyond our reach, this would imply that distributional inference in such traditions was impossible; but it is the principal aim of this thesis to shew that some progress can be made.

This concludes our discussion of the effects of III, IV and V on the construction of the stemma. We now consider their role in the application thereof.

Our reasoning on pp. 1:38 f. required assumption IV and was then capable of yielding unequivocally the reading of the archetype¹. If we now come to suspect IV, then we can no longer speak in terms of certainty, but only of likelihood; and our application of the stemma would be uncertain even if we were quite sure of the stemma itself. This was pointed out by Fourquet². Suppose three mss ABC, related according to Fig. B.2.14; in a certain passage, let AC agree in one reading, say x, while B has reading y.



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1. If IV was fulfilled but V was not, there would be some danger that a reading which we identified as that of the archetype was not "echt" (but it would nevertheless be "richtig", which is the important thing).
 2. Romania (1948-9), p. 94.

Without IV, we can no longer deduce immediately that x was the reading of Ω ; we have rather to set up two hypotheses:

- (a) Ω read x , and x was changed to y within the line $\alpha - B$.
- (b) Ω read y , which was changed to x once within the line $\alpha - A$, and once within the line $\Omega - C$.

If the likelihood of a change from x to y is roughly equal to, or greater than, the likelihood of a change from y to x , then we shall prefer (a); but if the change from y to x was far more likely (e.g. if y was a difficult reading for which x was an obvious though false correction), we would hesitate between (a) and (b), and possibly choose the latter. Fourquet concluded that it was safe to use this stemma only when x could be seen to be the lectio difficilior. But perhaps the role of the stemma is not as restricted as Fourquet suggests. We must bear in mind that the hypothesis that the archetype had the reading given by our critical rules, will generally postulate fewer changes than any alternative hypothesis (thus (a) involves one change and (b) two); and unless we can see good reason for supposing that the two likelihoods, of change in either direction, differ in such a way as to oppose our first hypothesis, we may accept the reading given by our critical rules as the more likely - but not as absolutely certain. Thus the process of deducing which reading goes back to the archetype, is analogous not to the solution of a (linear) algebraic equation but to the derivation of a maximum likelihood estimate.

We are left with the compound assumption VI. Of its three components, the first two would be granted in most traditions: that over a reasonably long text, every copyist will commit errors of transcription, some of which will persist (i.e. they will not be corrected away in descendants of that ms, nor undergo further variation) and will be in places where no erroneous reading (either the same or another) has originated in any other ms and survives among the mss now extant. This rests on the beliefs that all scribes are fallible, that the changes made in different transcriptions vary among themselves, and that some incorrect readings escaped notice or could not be satisfactorily improved. Given a sufficiently long text, we would usually be justified in assuming VIab; but we must be aware that results which depend on a very short sample of text may be invalid on that account¹.

Nevertheless, the validity of VIab has occasionally been challenged. Thus Fourquet maintained (p. 87) that it was never possible to prove that three mss were derived independently from an archetype (i.e. that the stemma had three branches); for we could always suppose that two of them, like AB in fig. B.2.14, had a common ancestor α ,

1. Hence one of the criticisms raised by Bédier (1929, pp. 41 ff.) against Dom Quentin's treatment of the tradition of the *Lai de l'Ombre*, viz that Quentin's sample of 68 lines out of 962 did not give adequate representation to all the ms groupings.

<p>Fig. B.2.15a from 1913, p.xxv</p>	<p>Fig. B.2.15b from 1913, p.xxix</p>
<p>Fig. B.2.15c from 1913, p.xxvi</p>	<p>Fig. B.2.15d from 1913, p.xxxvii; cf 1929, p. 40.</p>
<p>Fig. B.2.15e from 1929, p. 52</p>	<p>Fig. B.2.15f from 1929, p. 56</p>
<p>Note: O denotes the original; O^1 and O^2 are different editions going back to Renart. As in Fig. B.2.7, a seventh ms (F) has been left out. The marks (b) etc. in the first figure are explained below.</p>	

but that no errors were committed in the line $\Omega \rightarrow \alpha$, or all of them were corrected away, or none of them could be recognised by us to be errors¹. Again, Bédier suggested² that

1. This last of course involves the invalidity of VIc.

2. 1913, pp. xxxv f.

fig. B. 2.15c represented the relationship of the mss of the *Lai de l'Ombre* as well as did fig. B.2.15a. For we find that in four places (and nowhere else) ABCG agree in a reading inferior to that of DEF. According to (a), these four errors go back to an xca; according to (c) all our mss derive from x, which had those four incorrect readings (and apparently no other errors), but an xca of DE has emended them, so that DE now offer a text that seems to us unobjectionable but does not represent what Renart wrote. But that would imply that of all the errors committed along the line $O \text{---} x$, not one has persisted. Assumptions VIab may be open to serious doubt in the traditions with which Bédier and Fourquet dealt; but within other fields - I have in mind classical, biblical and patristic texts - they seem in general to be justified, and a scholar who had constructed a stemma would not be perturbed if he were shown a rival stemma that required him to believe VIab to be false.

Far more real are the doubts attaching to VIc, that we are able to specify which of the alternative readings is correct in enough passages for the orientation of the tree to be uniquely defined. This depends on the state of our knowledge, which sometimes is not enough to warrant the assumption. Thus Fischer, in a discussion on the textual criticism of the New Testament, refers to Froger's work, and states that if we were able to obtain an arbitrarily orientated tree, our ability to distinguish between correct and incorrect readings would not suffice

to determine the true orientation. "With a tradition as complex as that of the New Testament the network of relations will never be completely capable of translation into a stemma of manuscripts".¹ Again, Bédier's array of different textual histories of the *Lai de l'Ombre*, each having different implications for the editor, and each allegedly compatible with the textual data, requires a negation of VIc. Of the stemmata in fig. B.2.15, (a) was drawn up by Bédier in an edition of 1890; he believed that he had found common errors for each of the ms sets AB, ABCG, CG, DE. In a review of Bédier's edition, Gaston Paris would not accept that, in the five passages where Bédier had claimed that DE had a common error against ABCG, the reading common to DE really was inferior; thus Paris rejected the error set DE, and proposed (b). With (c) we have already dealt. Now E has a number of attractive readings; if we (with Paris) moreover doubt the error set DE, we may draw (d), which traces E back to a revised edition by Renart². Again, AB share a number of readings that "peuvent tenter plus que leur rivales" (1929, p. 56); if we believe them to be original, and, in addition, reject

1. J.T.S. (1970), p. 306.

2. If we formally define O^1 as the original, and O^2 as a copy thereof (as if Renart were acting as scribe rather than author), then this tree comes to be on the same footing as the others, its aim being uniquely defined, viz to reconstruct the text of O^1 .

the error set ABCG (for which Bédier adduced only four passages in justification), we shall arrive at (e).

Finally, we may decide that the truth is preserved sometimes in AB alone and sometimes in DE alone: this will lead to (f), which is not a stemma (in the strict sense) nor equivalent to any stemma. All these figures (except the last) are topologically equivalent to (a), and can be formed by "picking up" (a) at the appropriate points as marked. To a student unfamiliar with texts of this nature, the degree of uncertainty in choosing between different readings seems quite remarkable. Thus Bédier put forward four passages where the reading of ABCG, compared to that of DE, "apparaît nettement comme moins bon" (1913, p. xxiv); but the inferiority of these ABCG readings was not marked enough to deter him from proposing (e) and (f), in all seriousness, as rival stemmata which fitted the textual evidence just as well as (a). Where the merits of rival readings are so difficult to assess, we have no alternative to accepting that the final decision between two or more topologically equivalent stemmata is too delicate for us¹. But in many other fields, scholars seem to be more confident of their ability to recognise, in a small number of selected passages, which of two rival readings is original; and if they feel that VIc is justified, they will hope to be able

1. In his forthcoming book: "Numerical Taxonomy and Textual Criticism", J.G.Griffith gives an amusing example of an artificial text, differently preserved in six different mss, there being nothing at all to choose between the merits of the rival readings at any point. An editor faced with these mss would have to admit, as equally possible, some twenty different (but topologically equivalent) stemmata, and could not of course make any distributional inferences whatsoever.

to derive a stemma that is unique.

This completes our discussion of the six assumptions of Chap. 1. We have seen how each of them is liable to be invalidated, and how the fallacy of any of the assumptions may be either apparent (e.g. when we find it impossible to construct a satisfactory stemma) or not apparent (e.g. when we arrive at the stemma of fig. B.2.6a although the true history is given by fig. B.2.6b). In the latter case, we may well find that the stemmatic method gives results that are consistent both with one another and with the intrinsic criteria; and although there will always be a suspicion that our stemma does not reflect the historical reality, we shall do well to accept - albeit with caution - the implications of that stemma for recension. Stemmatic procedures, whether those of Greg, Maas or Froger, have produced results of great value¹, and may be expected to

1. The method of common errors, as associated with the names of Lachmann and Maas, has a great number of successes to its credit. A notable recent example is L.D.Reynolds, "The Medieval Tradition of Seneca's Letters", Oxford 1965. Reynolds was well aware of the dangers of contamination, but found it possible to speak in terms of stemmata, and indeed to draw them (e.g. pp. 19, 27, 31, 52). Again, Greg's method has proved its worth, particularly at the hands of Greg himself; the impact of his method outside the realm of English seems to have been modest, but within that field it has certainly won recognition. Thus Kane (p. 53, n.2) regards as "outstanding" Greg's reconstruction of the text of a work by Ben Jonson ("Jonson's Masque of Gipsies...An Attempt at Reconstruction by W.W.Greg," London 1952; the textual history is given on p. 70). Froger's procedures have not been available long enough to have been used so extensively; the only examples known to me are Froger's "L'Épître de Notker... in Études Grégoriennes (1962), pp. 23-71, and the study of S. C. Nita (reviewed on pp. 11:84 ff); see also pp. 11:56ff. I should think that Froger's method will go on to enjoy considerable popularity.

continue in the service of textual criticism. But when the attempt to reconstruct a stemma is manifestly unsuccessful, we must resign ourselves to the fact that our six assumptions, taken together¹, are far from realistic as applied to that case; we have seen, after all, that there are very many ways in which those assumptions may have been invalid. To insist on working with a stemma, in defiance of that fact, would be at best pointless, at worst misleading.

It would seem that in a tradition to which stemmatics could not be applied, distributional considerations would cease to be of any use in the establishment of the text; that "il n'y a pas de théorie, pas de règles et, partant, pas d'espoir de voir clair."² But this need not be so. In order for the ms groupings to admit the drawing of a satisfactory stemma, they must exhibit a highly specialised type of regularity; the majority of the variant passages must show one of a small collection of constant groupings, each of which obeys the consistency conditions of Lemma 6.

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1. It will not follow, of course, that all six must be invalid.
 2. P. Mertens, "Pour ou contre le «stemma codicum»?" (a discussion, in *Revue Belge Philologique*, 1965, pp. 72-78, on Dawe's Aeschylus book), p. 75.

But in cases where it is impossible to derive a stemma, there may yet be regularity of some kind within the data, even though that regularity falls short of what is required by stemmatics. Our aim now must be to try and detect a pattern within the textual data derived from a contaminated tradition, to find adequate means of expressing and representing that pattern, and to exploit it in unravelling the history of the text and in choosing between rival readings. To what extent these objectives can be attained, the reader may judge from the chapters that follow.

3. Data and Desiderata

We have seen that there are many textual traditions to which the stemmatic system cannot be applied; and at the end of the last Chapter it was suggested that other methods of analysis may prove helpful. Rather than proceed immediately to the technicalities of any particular method, let us in this Chapter first take stock, by considering, on the one hand, the properties of the data at our disposal, and, on the other, the sort of results we hope to obtain.

Our first reaction on surveying the distribution of different readings among the mss could well be one of bewilderment. True, there emerge a few constant groups, which, as we have seen (p. 2:45), do not carry us very far; and beyond that, hardly any regularity can be discerned. Let the reader consider, for example, the following list of the fifteen places in Cyprian's *De Unitate* where the mss group themselves either eight against ten or nine against nine:

TABLE B.3.1.

Page (Bevenot)	Reading selected for the text	Mss	Alternative reading	Mss
96	grassatur	[abBdekOpP]WY	grassantur	GhHJmpP]RT
97	in Christum	aBDhHmpTWY	in Christo	beGJKOPR
98	haeresis	[aBhHmRTWY]	haereses	bDeGJkOpP
102	iam	abBDhHJmpT	(omitted)	eGKOPRWY
105	nuntiat	aDehHJmpT	denunciat	bBGKOPRWY
106	Oportet	abBDhHmO	Oportet et	eGJkpPRTWY
107	Domini	aDGhHJmpRT	Dei	bBeKOPWY
109	medio	eHkmpPTWY	medios	abBDGhJOR
110	derelinquit	aDeGhHOpY	dereliquit	bBJKmPRTW
111	poterit	aDGhHmpRT	(words added)	[bBeJkOpP]WY
112	iniustitiam	aDehHmOpRT	iniquitatem	bBGJkPWY
117	ingemesimus	aDGhJORTWY	ingemiscimus	bBeH.kmpP
118	ad	abGJOPRY	in	BDehH.kmpTW
118	quaeritur	aBDeGHJOWY	quaeretur	bh.kmpPRT
123	(subscription added)	GJkmOpPRWY	(no subscription)	abBDehHT

Not one of these groupings is seen to recur at any other point in the whole treatise, and, taken together, they seem nothing short of chaotic. We shall need, first of all, to be assured that these arrangements are not altogether random and will bear analysis, and, secondly, to decide on some way of bringing out whatever regularity may be latent therein.

It goes without saying that statistical analysis will often reveal important trends and characteristics in data which seems, on the surface, quite unpromising. This requires a way of reducing the ms groupings to a numerical form. A course which has been taken by many workers in other fields, such as archaeology¹ and biology², is to set up coefficients of association (or divergence) between each pair of specimens³. This has in fact been tried by a number of workers in the field of textual criticism and two types of numerical coefficient between ms pairs have been devised:

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1. See e.g. W.S.Robinson's paper (cited on Thes., p. 3:20)
 2. Cf. R.R.Sokal and P.H.A.Sneath, "Principles of Numerical Taxonomy", San Francisco and London, 1963.
 3. An alternative method is Principal Component Analysis, on which, see pp. 11:152 ff.

1) Over a given domain, we determine the distance $d(A,B)$ between each pair of mss, i.e. the number of times that A and B offer different readings¹. An approach of this sort has been adopted by Dom Quentin², P. Canivet and P. Malvaux³, Griffith⁴, Froger⁵ and the Benedictine editors of the Vulgate Isaiah⁶.

2) We may instead calculate the connection $e(A,B)$ between each pair of mss, i.e. the number of times that A and B agree in the same incorrect reading. This has been tried by Bévenot⁷ and also by the Vulgate Isaiah editors⁶.

1. We assume for the moment that none of the mss is defective over the domain.
2. op. cit. (see Thes., p.1:5), pp. 227 f.
3. "La tradition manuscrite du $\Pi\epsilon\rho\iota$ τῆς θείας ἀγάπης ", Byzantion (1964), pp. 385-413.
4. "Numerical Taxonomy and Textual Criticism of a Classical Latin Author", Nature (July 1967), p. 326; "A Taxonomic Study of the Manuscript Tradition of Juvenal", Museum Helveticum (1968) pp. 101-138; "Numerical Taxonomy and some primary manuscripts of the Gospels", J.T.S. (1969), pp. 389-406. Griffith prefers to count the number of times each pair of mss agree over the collection of variant passages; this of course yields not the distance itself, but the difference between it and the total number of variant passages considered.
5. op. cit. (1968), pp. 129 ff.
6. Biblia Sacra iuxta latinam vulgatam versionem...cura et studio monachorum...ordinis Sancti Benedicti edita. Vol. xiii: Liber Isaiae, Rome, 1969. See Chap. III.
7. op. cit. (1961), pp. 124 ff.

Evidently (1) has a substantial claim to be preferred, for it depends only on the objective criterion of like against unlike. The investigator never has to decide which reading is the original one, or even to suppose that there can be only one original reading. The use of (2) however means that we must make those subjective decisions at the outset. Moreover, if we use (2) and subsequently appeal to our figures in choosing between variants, then we lay ourselves open to the charge of *petitio principii*; for our choice between readings depends on the statistics, which themselves depend on our initial choice between readings. There are ways in which the circular element can be controlled,¹ but, in the main, we must regard (1) as preferable in respect of methodology².

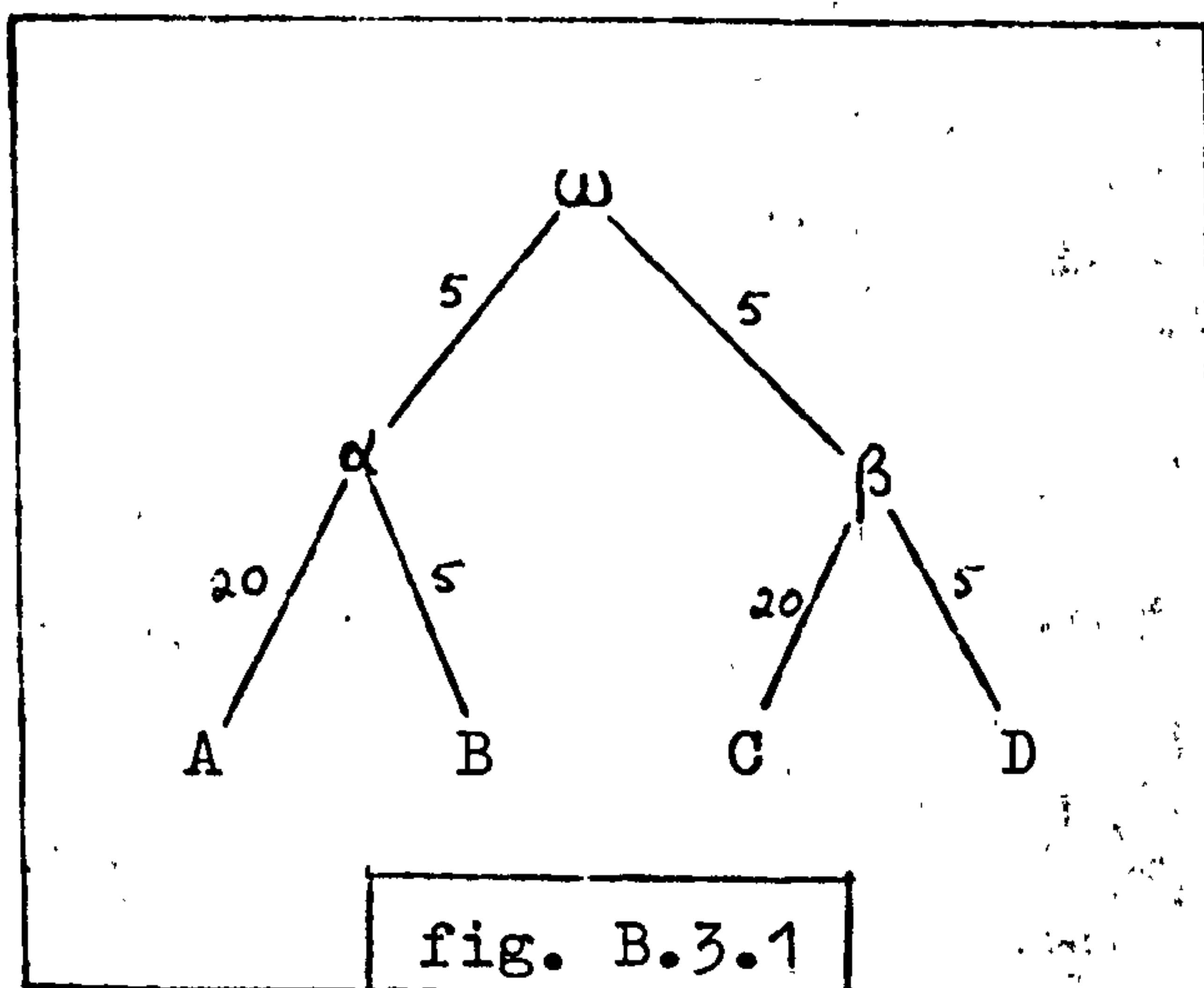
But with regard to interpretability, (2) seems superior; for the textual similarity between mss is not a sure guide to the degree of historical connection between them. Two mss may come to be textually similar for one of two reasons: either because their respective histories (or, more specifically, source-complexes) have much in common, as would happen e.g. if they were both copies of

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1. e.g. by basing our figures not on the whole work under analysis, but on a domain consisting of passages wherein we are more or less confident which is the original reading; it would then be legitimate to make inferences via our figures from that domain to other passages.
 2. Bévenot's work is considered again in Thes., pp. 11:113ff.

the same ms, or because they have each preserved with comparative fidelity, through two independent lines of tradition, the text of the original (or of some other remote common source). Thus a high degree of textual similarity may be due to either of, or to an unknown combination of, two quite different factors: genetic relationship, and fidelity of transmission¹. A high number of agreements in error, however, can be due only to the former. Consider, for example, the stemma of

1. An analogous situation is found in biological applications of numerical taxonomy. Sokal and Sneath are careful to distinguish between phenetic (i.e. having to do with similarity) and phyletic (i.e. having to do with community of ancestry) relationships. The sort of analysis expounded by them is based, of necessity, on phenetic resemblances only; and they point out (p. 52) that clusterings etc. carried out on that footing have no necessary phyletic connotations, and that in the great majority of cases, "the classifications are de facto phenetic ones, and they are phyletic by lip service and wishful thinking only" (p. 102). But they emphasise that in that field, a classification does not need to be phyletic in order to be meaningful and valuable (pp. 7 f.).

fig. B.3.1. Here α made 5 errors in copying ω , A made 20 in copying α , and so on; all 60 errors occur in different places.



The only mss now extant are ABCD. Then the distances are:
 $d(B,D)=20$; $d(A,B)=d(C,D)=25$; $d(A,D)=d(B,C)=35$; $d(A,C)=50$.
 Thus D is the most similar to B, but A is the most closely related¹. Connection measures, however, give a much more

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1. Thus Greg inveighs (p. 59) against the practice of deducing genetic relationship from textual similarity: "we may find it stated that of the three mss X, Y, and Z, the first two are the more closely related, and in explanation we are told that there are more readings common to XY than to any other pair. If this is all that is meant by the statement, it is a misuse of the term 'related'". Greg attributes this to what he terms the fallacy of constant variation, i.e. a tacit assumption that every transcription introduces approximately the same number of variants. I doubt whether that fallacy is entirely to blame, for it is not sufficient to warrant the procedure which Greg rightfully condemns. Thus let us assume that every transcription introduced five errors. Now if there were three arc mss along each of the arcs α -A and β -C, we would still have a situation in which similarity did not altogether reflect historical connection. The mistaken deduction of the latter from the former seems rather to be a heresy in its own right.

helpful picture: we have $e(A,C)=e(A,D)=e(B,C)=e(B,D)=0$, $e(A,B)=e(C,D)=5$, which plainly brings out the genetic relation between A and B, and between C and D. Again, the editors of the Vulgate Isaiah seem to have found that the connections give more satisfactory results than the distances; having drawn up two tables, according to each of the two approaches above, in order to discover the relationships between some primary Vulgate mss, they observe (p. xxxiii): "Constat historiam textus altera tabella lucidius exprimi quam priore." Nevertheless, although (2) is in this respect tempting, method demands that we concentrate our efforts on (1).

Thus, if we are given the readings of m mss, we may obtain the distance between each pair - of which there are altogether $\frac{1}{2}m(m-1)$. Some properties of the distance function thus defined are of interest to the mathematician. The three "metric" properties of ordinary distance, first considered abstractly by Fréchet in 1906, are fulfilled: if $d(A,B)$ denote the distance between the mss A and B, then

- (i) $d(A,A)=0$, $d(A,B) > 0$ if A and B are distinct¹;
- (ii) $d(A,B)=d(B,A)$;
- (iii) $d(A,B)+d(A,C) \geq d(B,C)$ (Cf. n.2 below)

1. Compare Lemma 7 (p.1:26)

2. Thus any three mss may be represented by three coplanar points, whose inter-point distances equal the inter-manuscript distances. It is interesting that we cannot generalise this, representing m mss by m points in real $(m-1)$ -dimensional space. Thus consider four mss ABCD over a domain of two splits, $AB:C:D$ and $AC:BD$. Then $d(A,B)=d(A,C)=d(B,D)=1$; $d(A,D)=d(B,C)=d(C,D)=2$. The reader may verify that we cannot set up four points ABCD in real three-dimensional space with those inter-point distances (that is, Euclidean distances).

The conversion of our list of ms groupings to numerical form involves a loss of information; for that list defines a unique table of inter-manuscript distances, but one table may correspond to more than one list of groupings. Thus consider the two following lists of splits, for four mss ABCD in six passages:

I	II
A:B:C:D	AB:CD
A:B:C:D	AB:CD
A:BCD	AC:BD
ACD:B	AC:BD
ABD:C	AD:BC
ABC:D	AD:BC

Every pair of mss will yield exactly four divergences over the six passages, whether we work with the groupings of list I or of list II. Accordingly, the two lists will give the same table of ms distances (each entry being 4). But naturally this information loss will be outweighed if we can trace in the distance table a pattern that was not evident in the "raw" data.

The definition of distance which we have employed here, requires that every ms reading be known at every point of the domain. In practice, however, we often find that one or more of the mss is for some reason deficient. Thus in the study of the Peshitta Psalter I wished to describe 21 witnesses, each of which contained at least the major part of Psalms; but had I restricted myself to places in which the readings of all 21 survived, I would have been left with only 36 places out of a total of 447 in which at least the majority of the witnesses were available. It therefore

seemed expedient to work with the percentage distance between two mss, defined as the ratio of the number of passages in which the two mss diverge to the number of passages in which the readings of both are known. These percentage distances are more cumbersome but more generally applicable than absolute distances¹.

We note in passing a useful convention in the recording of certain complex variants. Consider six mss A...F, which offer the readings:

if he was	AB
if he were	CD
if he became	EF

To treat this as AB:CD:EF, and thus to obtain 100 as the (percentage) distance - over that passage alone - both between AC and between AE, is unsatisfactory in that the reading AB is closer to CD than to EF. It seems better to resolve² the variation into two splits:

ABCD:EF	be/become
AB:CD (neglecting EF)	was/were

In the second split, we behave as if EF were not available; they are not relevant to the opposition in question,

1. Similar difficulties are met with in biological taxonomy, when a character used for comparison is not present in one of the specimens under analysis (Sokal and Sneath, pp. 162 ff.).
2. For a detailed discussion on the resolution of complex variants, the reader is referred to Greg, pp. 30-43.

was/were. Thus, over that passage alone, the percentage distances are: $d(A,C)=50$, $d(A,E)=100$, which gives a more satisfactory account.¹

The question of the processing of these distances is dealt with in Chap. 4; but first we must ask ourselves which variant passages are to be admitted to the analysis. This may be dealt with on more than one level. There is on the one hand the character of the variations themselves. As Greg has pointed out (pp. 17 f.), the danger of restricting ourselves to major variations only is that we may limit the field to too narrow a selection of variants; whereas if we take into account too many minutiae, we run the risk of including much "grit" - minor features (of spelling, grammatical form etc.) in which scribes tended to be led by their own habits rather than by what they saw in the exemplar. Particularly dangerous are features which tend systematically to group together certain mss regardless of their historical relationships. Thus, if we admit a great number of passages in which some mss have ancient and others modernised spellings, this will tend to bring together mss which were written by conservative scribes against those which were not, and if there were a large number of such instances, the true groupings would be contradicted and obscured systematically. This borne

1. However, it would not be worth while to generate a second variant in this way if the majority of the mss had to be treated as if they were not available.

in mind, the investigator must decide for himself which variants deserve to be included.

A question which follows on from the last, is whether we ought to "weight" the passages, in that the matters in which the textual variations lie are of widely differing degrees of importance. This does not seem desirable¹, for many reasons. We shall immediately introduce a considerable subjective element in setting up a scale of importance of variations. Moreover, the fact that a variation lies in a particularly important feature does not necessarily mean that it will be of particular value in the study of the textual history; for it is especially on major points that a scribe tends to look to sources other than his exemplar. Greg goes so far as to write (p. 57) that "where conflation is suspected, the value of variants as an indication of ancestry is in inverse proportion to their intrinsic importance" (my italics). It seems then that once the editor has decided which variants to work with, he should count them all equal in drawing up his figures. I would add that other investigators, both in textual criticism² and in biology³, do not recommend the use of weighting factors.

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1. For an instance in which I felt it to be the lesser of two evils to resort to weighting, see p. 7:20.
 2. Griffith decided against using them in both his investigations mentioned above (1968, p.114; 1969, p.390). He told me that he had drawn up figures for Satire X of Juvenal with and without weighting factors, and had found no substantial difference either way. Dr. Dawe also suggested to me that weighting had little effect.
 3. Sokal and Sneath discuss the problem at length (pp. 118 ff.). Much of their argumentation is valid in relation to textual criticism as well as their own discipline. They conclude (p. 120): "Equal weighting can...be defended on several independent grounds; it is the only practical solution, it and only it can give the sort of natural taxonomy which we want..."

Intrinsic considerations are not the only criteria for deciding which variants are to be used in compiling the inter-ms distances. An important question is whether cases in which one ms departs from all others should be included. I advocate that they should not, because they tell us only that one ms diverges from the rest, and seem incapable of shedding light on the relationships of the mss to one another. This does not imply any value judgment, that readings found only in one ms are on those grounds not significant; indeed it may often happen that the original reading is preserved in only one ms¹. Again, a reading can be said to be unique to a certain ms only in relation to the other mss collated; a change in the selection of mss for collation may well alter the case². It is solely on the grounds that these unique

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1. See Thes., p. 7:41, and references cited at the foot of that page.
 2. A striking illustration of this may be seen in the history of Peshitta criticism. Cornill, in his commentary on Ezekiel, collated the Ambrosian codex (7a1,A) together with a number of printed texts; finding that A offered several readings which were not attested by any of those printed editions, and which moreover agreed with the Hebrew, he concluded that A had been conformed to the Massoretic Text, and that "among all accessible texts of the Peshitta, A is the worst" (p. 145). However, Barnes collated nine other mss and not only discovered that A's "isolated" readings were usually supported by those nine, but also shewed that all the printed texts used by Cornill depended on a single late ms (17a6,z). Thus in most of the places wherein Cornill accuses A of a peculiar aberration, that charge would be more justly brought against z. See Barnes! Chronicles book (cited p.7:71), pp. xxii ff. Cornill subsequently retracted his opinion in favour of that of Barnes.

variants seem to have no bearing on the inter-relations of the mss collated, that I prefer - again in the company of other investigators¹ - to disregard them.

Another question is: Ought we to set up a minimum frequency which a grouping has to attain in order to be included in the analysis? It could be argued that rare groupings, and in particular the considerable number of them which are attested only once (such as those of Table B.3.1), seem to exhibit no regularity at all, and would tend if anything to obscure any order which might otherwise be traced in our data. Perhaps therefore we should exclude any ms grouping which is attested less than a certain number of times (e.g. twice). I investigated this point as follows. Taking as a domain the 164 Cyprian passages shewing a two-way split of a type not attested at all elsewhere, I computed the absolute distance between each of the 153 ms pairs which could be formed from the eighteen mss. The distribution of these 153 distances is given in a histogram in fig. B.3.2. I then generated a second list, also consisting of 164 two-way splits, in the following way. Wherever the mss were observed to diverge with, say, three mss against fifteen, I used pseudo-random numbers to form a random split, by choosing at random three mss out of the eighteen. Thus, to correspond with every place where the

1. So Griffith (1969, p. 397; see however Thes., p.11:134).

mss grouped themselves, m against $(18 - m)$, there was formed a "randomised" split of the mss; m against $(18 - m)$. The 153 distances over those 164 random groupings were then computed. I then generated four other random lists, each of 164 ms groupings, and drew up tables of the 153 distances for each list. The first five entries in the lists themselves are shewn in Table B.3.2.

The observed groupings, examined singly, are not obviously different from the randomised ones. This would at first suggest that the groupings which are attested only once are virtually random, and ought to be excluded from our computations. However, when each of the six lists of Table B.3.2 is converted to a table of inter-manuscript distances, an important difference can be readily observed between the first and the other five: the distances due to the true groupings are spread over a wider range than those due to the randomised ones. This may be seen from the histograms of fig. B.3.2, and from Table B.3.3. on the six sets of distances.

Thus the contribution of the true set of once-variants to the table of distances is markedly different from that of a set of random groupings. This implies that even variants which occur only once are not "grit", and may be admitted. It also shows that although a contaminated tradition will give rise to ms groupings which exhibit no apparent order and which lead the investigator to give up hope of making any use of distributional data, there is nevertheless a certain structure, which is revealed in the table of distances.

TABLE B.3.2.

Variation (see text)	Observed grouping ¹	Randomised groupings				
		I	II	III	IV	V
subruendis - subvertendis	bhHmpRT	akOpTWY	bHJkOTY	behHPTY	BeGHJmO	BDGmPTY
ac deiciendis - om.	ap	Jp	pY	aJ	BT	eW
grassatur - crassatur	abBpp	GhHWY	bKOWY	bBeTY	aeHJT	DpPTW
grassatur - grassantur	GhHJmPRT	aBhHJOPY	abDmOpPR	bBeGJOpY	bBGhJKOR	beGkmRWY
obrepit - obripit	aeJ	GOW	DHY	aDY	bKT	BpW

TABLE B.3.3.

Distances based on		Distances based on random groupings				
Observed groupings		I	II	III	IV	V
Least member	17	32	42	44	39	42
Greatest member	85	79	73	73	74	72
Standard deviation ²	10.55	6.83	6.81	5.45	5.74	5.43

1. To save space, only the smaller of the two groups into which the mss divide is given.

2. All six lists had the same mean, i.e. $\sum s_i (18-s_i) / 153$, where s_i and $18-s_i$ are the sizes of the groups into which the mss divide themselves in the i th passage (i-runs from 1 to 164); this mean was 57.76.

TRUE DISTANCES

'RANDOMISED' DISTANCES

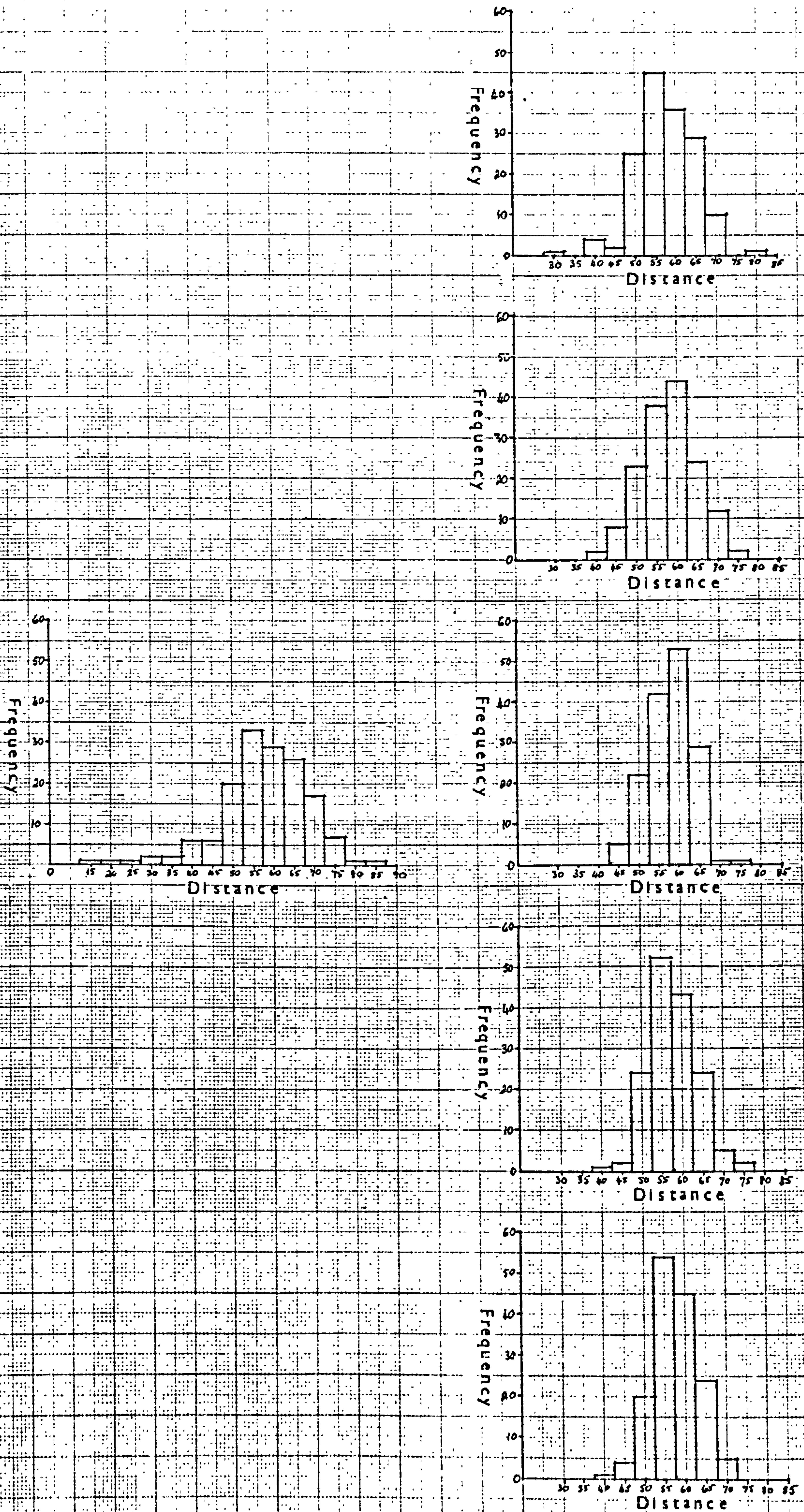


FIG R 3 2

Having dealt with the selection of the variants for analysis, we now ask: How long a passage ought we to take? One might have thought that the larger our domain, the more reliable our results. However, we remarked on p. 2:9 that we must be aware of the possibility that the textual history changes in the course of a work, by virtue of changes of exemplar among the mss of the source-complex. If our domain is too extensive, we shall lose sight of these changes. Thus we have to consider the manner in which the textual history varies.

Some conclusions may be drawn from the following experiments. I divided into sections, each containing about the same number of variant passages, my data for four of the textual traditions on which I have worked¹:

Cyprian: De Unitate	-	4	sections
Aeschylus: Persae	-	4	"
Gospel of Luke (samples)	-	3	"
Peshitta Psalter	-	4	"

For each section, I constructed a table of distances². I then computed, for each possible pair of sections from the same work, the correlation coefficient between the two series of distances derived from those two sections. These correlation coefficients are given in Table B.3.4. The symbol \longleftrightarrow appearing in the table will be explained shortly.

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1. For information on my sources etc., see pp. 4:12 ff.
 2. In the first three traditions, each section yielded about 70-100 variants in which all the mss under analysis were fully reported, and thus I was able to use absolute distances. But for the Peshitta Psalter, percentage distances were necessary.

TABLE B.3.4.

(a) Cyprian (18 mss)

SECTIONS	1	2	3	4
1		.76	.73 ↔	.75
2	.76		.83	.80
3	.73	.83		.75
4	.75	.80 ↔	.75	

(b) Aeschylus (16 mss)

SECTIONS	1	2	3	4
1		.55 ↔	.55	.53
2	.55		.61	.57
3	.55	.61		.78
4	.53	.57	.78	

(c) Luke (14 mss)

SECTIONS	1	2	3
1		.83 ↔	.84
2	.83		.87
3	.84	.87	

/continued

Table B.3.4 (Continued)

(d) Peshitta Psalter (21 mss)

SECTIONS	1	2	3	4
1		.71	.67	.63
2	.71		.82	.80
3	.67	.82		.76
4	.63	.80	↔ .76	

We first observe that every entry is positive and moreover exceeds 0.5. This demonstrates the resemblance between data gathered from different parts of the same text, and suggests that as we proceed through the text the ms inter-relations change only gradually, and are not transformed out of recognition. It also provides further evidence of the underlying regularity in the data.

The correlation coefficients moreover exhibit a pattern which has been well-known since the publication of an important article by W.S. Robinson¹. As we follow any row of the table, we find that the correlation coefficients usually decrease as we go away (in either direction) from the leading diagonal. Exceptions to this tendency are marked thus:

↔ . We thus find that section 1 of the Peshitta

1. "A method for chronologically ordering archaeological deposits", American Antiquity (April 1951), pp. 293-301. See also "The place of chronological ordering in archaeological analysis", on pp. 301-313 of the same journal, by G.W. Brainerd, who collaborated with Robinson.

Psalter resembles section 2 the most, section 3 somewhat less, and section 4 less still. That is, these four sections form a graded series, of which each member is closer to its neighbours than to more distant elements.

That Table B.3.4 does follow that pattern to a significant extent - even though we never have to deal with more than four elements, and Robinson's examples all involve at least eight - cannot be reasonably doubted, although I have been unable to set up a formal test of significance. Thus let us define r_{ij} as the correlation coefficient between sections i and j . If we are comparing n elements, a perfect "Robinson model" will satisfy altogether $(n-2)(n-1)$ inequalities ($r_{12} > r_{13}$, $r_{13} > r_{14}$, etc.). This is not often met in practice, as Robinson himself observes; some of the differences which we expect to be positive (e.g. $r_{12} - r_{13}$, $r_{13} - r_{14}$) may turn out to be negative. But if the number and magnitude of these "negative differences" seem unsubstantial in comparison with the others, then the fact that the elements being compared form themselves into a series¹ is still meaningful; and an examination of Table B.3.4 will show that in no case are the negative differences considerable.

1. This is not, of course, to be confused with the work of Griffith, who has arranged in a series the manuscripts themselves, not the different parts of the text.

There seems then to be a systematic tendency, at least in some cases, for sections of a work which lie close together in respect of their position in the text to show a particularly high degree of correspondence in the behaviour of the mss. How is this to be interpreted? Robinson was concerned to shew that if a collection of archaeological deposits could be arranged thus as a series, that series would give the chronological order of the elements. Here, however, the factor underlying the progression as we go through the series, is not the passage of time; it seems rather to be the continual changes of affiliation (in the sense wherein the term is used on p. 2:9) which took place among the mss of the whole source-complex. In other words, as we proceed through even a reasonably short text - such as the 746 lines of the Persae on which Table B.3.4(b) is based - the background of manuscript inter-relations, reflected in the distance tables, is gradually changing.

The effect of this instability will sometimes be evident, when a change of exemplar occurs in an extant ms (or a close ancestor thereof)¹. But changes of allegiance in such mss are, I believe, far less numerous than in those mss of the source-complex which are lost and relatively

1. Of this an outstanding example is the "Protean" behaviour of G in Juvenal; see Griffith (1968), pp. 136 ff.

remote; for the extant mss constitute only a small proportion of the source-complex. Thus I imagine that the principal effect of ms instability is a gradual change of climate, as it were, in the course of the work, rather than (or in addition to) a series of obvious metamorphoses.

What are the implications for the question of the length of the passage to be selected for analysis? The number of changes of affiliation which have brought about this instability seems to have been large; we cannot hope to determine all the points at which these changes took place, and to divide up the text accordingly into a series of small domains, each with a textual history of its own. On the other hand, the fact that even the farthest sections of the works considered above show a fairly high correlation (over 0.5), suggests that although many of the ms relationships did alter, many - and probably the majority - remained unchanged. Thus it seems best to treat (at least initially) the entire text as a single unit, from which the ms distances are to be derived; this gave, for the texts studied here, collections of about 400 variant passages. The study of the behaviour of the mss in particular portions of the work must then be deferred until we have considered the text as a whole.

This concludes our discussion on the properties of our data and on their implications for our studies. We now ask what we hope to achieve from our investigation.

Firstly, we must try to obtain procedures whereby an editor may exploit his knowledge of the distribution of each of the rival readings, in his choice between them. We have already seen that even when a valid stemma can be reconstructed, the criteria of distribution will not suffice to guide us with certainty to the true reading (pp. 2:50 f.). This will be true a fortiori in a case wherein no stemma can be established. There is no question of our arriving at invariable rules, for recognising the true reading from the distributional evidence; as Housman¹ observes, the aim of such a rule would be "purely humanitarian; to rescue incompetent editors alike from the toil of editing and from the shame of acknowledging that they cannot edit". But the fact that such considerations do not in themselves lead to certainty, does not mean that they are of no value whatsoever. Whether or not we are able to draw up a stemma, it is surely important that our judgment should be based not only on the nature of the conflicting statements that are offered to us, but also on the character of and the relationship between the witnesses that support those different statements. Thus we must try to ascertain the relevance of distributional evidence.

Our second desideratum is a history of the text. True, this will not in itself interest the editor qua editor. Nevertheless, our confidence in any critical

1. "D. Iunii Iuvenalis Saturae, editorum in usum edidit A. E. Housman", Cambridge 1931 (2nd ed.), p. xiii.

policy that makes use of distributional evidence will be greatly enhanced if we can outline the course whereby the text was (or may be deemed by virtue of the equivalence axiom to have been) transmitted, and which would provide a rationale for that policy. Needless to say, the readings of the mss alone will not yield a textual history that is unique. However, external evidence may perhaps enable us to narrow down the possibilities¹; and if it then proves possible to sketch the history of the text, we shall have forged a useful instrument.

A third objective is to reduce the labour of collation. Froger has suggested that in traditions which yield no stemma, it may nevertheless be possible to identify clusters ("nuages") of mss which particularly resemble one another, and that we could then work with just one member to represent the whole cluster (p. 135). Again, we may follow Bévenot² in trying to identify a small "team" of mss, each of which derives from the original by a route that is largely independent of that of the other members of the team. At any point in the text where the true reading is preserved by some of the extant mss, it will be unlikely that all the mss of our team will present an error. Thus we should be

1. See for example pp. 9:32 ff.

2. Cf. his paper of 1970 (Thes., p. 11:113).

able to work with a small selection of the mss, in the knowledge that almost every extant reading that may claim to be original will be represented among them. This will greatly ease our burden, even if we do not succeed in formulating a distributional policy for cases in which the members of the team differ among themselves.

We may note in passing an important difference of emphasis between textual criticism and biological applications of numerical taxonomy. In the latter, as expounded by Sokal and Sneath, the classification of specimens is regarded as an end in itself, whether or not we discover the history of their evolution and, in particular, the properties of their common ancestor. "A basic (and very controversial) attitude of the proponents of numerical taxonomy is the strict separation of phylogenetic speculation from taxonomic procedure" (p. 55). However, the specimens studied by the textual critic, i.e. the manuscripts, are of interest only as carriers of the text. If the reconstruction of the original text is his primary objective, classification of the mss by itself will not carry him very far unless it holds out some prospect of leading back to the original. Thus despite the obvious analogy between biological and textual evolution, the textual critic has special requirements, and classification for its own sake is not one of them. The main objectives, then, which the textual critic would find useful enough to justify the labour of analysis, are the three outlined above.

We now attempt to see how far we can make use of our data to attain those desiderata.

4. The Method Chosen: Multidimensional Scaling

In the problem which he now faces, the textual critic is by no means alone. He is dealing with a set of elements, of which he has supplied a measure of the degree of divergence (or similarity) between each pair, and he is trying to represent whatever structure is embodied in his data. Analogous problems arise in many fields, as we have already stated (p. 3:3), and a number of approaches have been evolved, to meet the needs of different investigators¹. We are now called upon to choose which is the most promising as applied to the problems of textual criticism.

When the analysis has been completed, according to the different techniques that I have encountered, the elements are usually set out in one of the following three forms:

(a) a tree. This will result not only from attempts to trace the evolutionary history, but also from any method which sets up any sort of hierarchy (e.g. cluster analysis²). Such a tree will resemble the stemma in form, but not necessarily in function.

1. An excellent account of some of the principal methods available is given by G. L. Cowgill, "Archaeological Applications of Factor, Cluster and Proximity Analysis", in *American Antiquity* (July 1968), pp. 367-375. The textual critic too will readily understand Cowgill's article, which requires no specialised knowledge of mathematics.

2. For an explanation of this term, see Cowgill, pp. 369 f.

(b) a spectrum, i.e. a listing of the mss in an order such that those which most resemble each other are closest together, and those which are relatively dissimilar are placed further apart. This type of analysis is known as seriation and was developed particularly in Robinson's paper of 1951 (see Thes., p. 3:20). It has been introduced into the field of textual criticism by Griffith, who has used it to great advantage.

(c) a map (in any number of dimensions, as specified by the investigator) on which each ms is represented by a point, and the distances between points reflect the divergence measures between the corresponding mss.

I have considered each of these methods, and applied them to different sets of data. My main results are presented not here but in Chap. 11, since I did not wish to interrupt unnecessarily the exposition of the approach which I have come to regard as the most suitable, that is (c). There are many reasons for this choice. One is that the map seems to convey far more information than either the tree or the spectrum; this question is examined in detail on pp. 4:19ff. Another is that it is possible to interpret the map in such a way as to obtain meaningful distributional inferences; I have not been able to do the same with either the tree¹ or the spectrum.

Two methods are available to generate maps: principal component analysis, and multidimensional scaling (MDS).

1. that is, when it is impossible to draw a stemma, and we have only a "best-fitting" tree or the like.

The special characteristics of our data create conditions which do not, in my opinion, favour the former method (see pp. 11:152 ff); and so we are left with MDS¹.

For a more detailed idea of the workings of MDS, we turn to the following account by one of the pioneers who developed it, J. B. Kruskal²: "Suppose we have a map of Europe with various principal cities marked on it..... It is a simple matter using a ruler to measure the distances between all the cities.... The result is a table of inter-city distances. MDS can be thought of as the reverse process: start with the table of inter-city distances,

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1. I am grateful to Rex Galbraith, of the Department of Statistics, University College London, for first telling me of this technique. The articles which bear on the work of this Thesis are: R. N. Shephard, "The Analysis of Proximities: Multidimensional Scaling with an unknown Distance Function". Part I: Psychometrika (1962), pp. 125-140. Part II: *ibid.*, pp. 219-246. J. B. Kruskal, "Multidimensional Scaling by optimising goodness-of-fit to a nonmetric hypothesis" (same journal, 1964, pp. 1-28) and "Nonmetric multidimensional scaling: a numerical method" (*ibid.*, pp. 115-129). Though I have referred to Shephard, it is Kruskal's method that I have followed.
 2. The following is taken from an abstract of a paper by Kruskal which was read at the Anglo-Rumanian conference of 1970 (see ref. on p. 11:84). For supplying me with this abstract I am grateful to Dr. F. R. Hodson.

and make a map showing the position of the cities. By using sophisticated numerical methods and a large computer it is usually possible to reconstruct the map fairly accurately, even though (1) the table contains random errors which are quite large, (2) several entries are missing, and (3) all the entries are distorted in some unknown (but systematic and common) manner. Of course, the reconstruction is subject to certain inherent limitations. It is not possible to tell which way North points, so that the reconstructed map may be turned sideways. Also, the scale of the reconstructed map may be different from that of the original. The most interesting applications of MDS occur in situations where the very existence of a 'map' is not clear in advance".

Thus, if we can supply the degree of divergence between each pair of mss, MDS will allow us to derive a map. The rest of the present chapter will be devoted to a discussion of the questions which have arisen in the application of this method in the field of textual criticism. Much of this discussion is unavoidably technical, and acquaintance with Kruskal's papers is assumed. No special mathematical knowledge is required, however, in order to "read" and interpret the map; such questions are dealt with in Chapter 5.

1. TECHNICAL PROBLEMS IN THE APPLICATION OF MDS TO TEXTUAL CRITICISM

Kruskal leaves open a number of decisions for the investigator, who has to consider the special properties of his own data. The first is the number of dimensions in which the map is drawn. Naturally, the more dimensions, the more freedom to locate the mss so as to minimise the stress¹. However, a map in even three dimensions is not easy for everyone to visualise, and if four or more dimensions were involved, the results would hardly be illuminating to the average textual critic. I first thought it best to work with three dimensions, and in fact constructed six of my maps on that basis. However, when I came to show my results to recognised textual critics (who had had, reasonably enough, no special mathematical training), these three-dimensional figures were often greeted by an attitude of polite bewilderment which soon convinced me of the expediency of reducing the number of dimensions to two. Only in one case² did this change increase the stress to an extent that was at all alarming, as the reader may judge from Table B.4.1 (p.4:17); and all the two-dimensional maps had a stress of less than 20%, which Kruskal considers sufficient to give a meaningful map³.

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1. a measure which is associated with any map and expresses the extent to which the map fails to correspond with the data.
 2. viz the Persae of Aeschylus.
 3. Kruskal has set up (p.3) the following empirical scale for evaluating from the stress the goodness of fit:
20%, poor; 10%, fair; 5%, good; 2½%, excellent; 0%, "perfect". The stresses for the 2-D maps I have obtained range from 7.5% to 16.0%.

Kruskal moreover makes provision for two approaches to ties, i.e. cases wherein two of the data distances are equal. Should we "penalise" the map (by an increase in the stress) if the two corresponding map distances are not also equal, or should we not? Kruskal terms the former policy the secondary approach to ties, and the latter the primary. I have thought the so-called secondary approach to be preferable. In any case, when percentage distances are used, ties are rare.

The investigator is also free to choose the distance function to be employed in calculating the distance between the points on the map. This need not be Euclidean distance but any Minkowski r -metric. I have experimented to a small extent with such r -metrics (where $r \neq 2$), but I could find no advantage in them to compensate for the difficulties of comprehension which would then confront the general "literary" reader.

There is one special difficulty which is apparently inherent in MDS as expounded in the above-mentioned papers of Shepard and Kruskal, and which has come up in one of the maps here constructed. It is that under certain circumstances we may obtain a "trivial" solution. Suppose it is possible to divide the set of mss into a small number (say two or three) of subsets in such a way that the distance measures for all pairs of mss within the same subset are smaller than the distance measures for all pairs that are divided between different subsets. Then we are in danger¹

1. Whether the danger materialises will depend on k , the number of subsets, and t , the number of dimensions. The condition for a trivial solution appears to be $t > k-1$.

of obtaining a configuration with minimum stress (possibly zero stress) in which all the members of each subset are represented by a single point. (Thus our map will consist of only k points.) A particular case, with $k=2$, is considered by Shepard (p. 240), who suggests that we either construct a separate map for each subset or have recourse to some alternative method. The form which the difficulty took in my own work was, that in an attempt to construct a map for 14 witnesses of St. Luke's Gospel¹, the mss divided themselves into three such subsets and yielded a map of only three points². Rather than use alternative methods of analysis, I proceeded as follows. Suppose we have n mss M_1, M_2, \dots, M_n , which yield a trivial map. Let us now consider an additional hypothetical ms M_0 , defining its distance $\delta(M_0, M_i)$ to each of the n mss to be zero. This ms M_0 is of no significance in itself, but if we add it to our collection of n mss, it will no longer be possible to partition the mss into subsets so as to yield a trivial solution. When a map for these $(n+1)$ mss has been found, M_0 can be discarded; the stress for the configuration of the n mss that remain is then computed, and we may then proceed as if that difficulty had not occurred.³

1. For details, see below (p. 4:14).

2. The three groups were: (1) A, E, S, W, Θ , Π , Y, fam 1, fam 13; (2) Δ , B, L, ρ^{75} ; (3) D. I later found that these groups corresponded to divisions customarily made by scholars, in that (1) consists of Caesarean and Byzantine, (2) of Alexandrine, and (3) of Western texts.

3. Perhaps this device will be of service in other applications of MDS.

Once the map has been derived, its scale and orientation can be determined by us as we please. However I have thought it expedient to adopt the conventions of (i) fixing the origin as the centre of gravity of the configuration of points, and (ii) referring the configuration to its principal axes. There seemed to be little advantage in specifying the scale¹, which is best determined on grounds of convenience.

It may happen that, after the map has been completed, we wish to locate an additional ms, or a fragmentary ms which is available in so few places that we must be somewhat diffident about its distances from the mss which were considered when the map was first derived. Let us denote this new witness by N , and let m be the number of mss already on the map. If we can compute some or all of these distances, then we could of course go back to the beginning and construct a new map; but this would probably entail an unacceptable waste of computer time², and would moreover give

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1. I first wished for a scale such that the mean of the data distances was equal to the mean of the map distances; for I hoped that the two sets of distances would then more or less correspond. It turned out, however, in every case, that the map distances were then spread over a considerably greater range than the data distances, and so that correspondence was not achieved.
 2. On the amount of computer time used in drawing up the map, see p. 4:16.

a result which involved in some degree the distances between N and the other mss, quantities which might be somehow questionable¹. I have therefore preferred to estimate the location of N in one of the following ways:

(1) If we consider the relative proximity of N to each of the other m mss, it may be possible to estimate by inspection where N should lie. This is the most straightforward method when N seems to be within an area where the mss are more or less clumped together.

(2) More formally, we may seek a location for N such that the map distances between N and the other mss correspond to the data distances. This may be done as follows. We have already obtained the map and data distances (denoted in Kruskal's papers by d and δ respectively) between the mss now on the map. Let us now derive, from those two corresponding sets of distances, the least square regression line of d on δ . This will yield a linear equation²

$$d = A \delta + B.$$

This will allow us to estimate d , the map distance, when we are given δ . Let us now consider the actual distances between N and the other m mss; they may be denoted $\delta_1, \delta_2, \dots$. We now define $\delta'_i = A \delta_i + B$,

1. e.g. if N were a hypothetical ms whose readings were only inferred, not known.
2. I have found in every case that B is negative. This seems to be connected with the fact that the coefficient of variation over the set of map distances was found to be greater than that over the observed distances.

for $i = 1, 2, \dots$. What is now sought is a location of N such that the map distances between N and the other m mss are as nearly equal as possible to these quantities δ' . If those other mss are denoted M_1, M_2, \dots , and if $d_i(x, y)$ represents the distance from any point (x, y) on the map to the point which represents M_i , then we shall select as our location for N that point which minimises

$$S_1 = \sum_{i=1}^m (d_i - \delta'_i)^2$$

(3) It may happen in traditions wherein the mss M_i are themselves deficient to a considerable extent (though not enough to merit being treated as fragments), that some of the quantities δ_i are based on a rather greater number of passages than are others. Thus in Chap. 8 an attempt is made to locate on the map the Psalm texts used by a number of ecclesiastical writers, on the basis of the passages which they quote. From pp. 8:45 f. the reader will pick out the figures for one of these writers, Aphraates. There are fifteen passages which he quotes, and which may enable us to locate his text on the map. Some of our mss of the Psalms are extant in all fifteen places; others give as few as six. It could well be argued that the terms in S_1 that correspond to mss in the latter category should be allowed less weight than those in the former. The position is similar for the other writers in Table B.8.1; thus the percentage distance of 50 between Daniel and G (a ms with many gaps) is based on only two passages, while the distance of 31 from Daniel to Cod. A (which is in an excellent state

of preservation) is based on sixteen passages. When all the mss used for the map are well preserved, such a discrepancy will not arise; but when they are not, it is best to weight the terms of S_1 according to the number of passages over which δ_i is calculated. Thus let Δ_i = no. of divergences between N and M_i , and let E_i = no. of variant passages in which N and M_i are both available. Then $\delta_i = 100\Delta_i/E_i$. We may now seek to locate N in such a way as to minimise

$$S_2 = \sum_{i=1}^m \{ E_i (d_i - \delta'_i) \}^2$$

which may also be expressed as

$$\sum_{i=1}^m (d_i E_i - 100A\Delta_i - BE_i)^2$$

There is a certain theoretical awkwardness in the last two methods. In establishing the original map, we employ not the distance figures themselves but only their rank ordering. Methods (2) and (3), however, utilise the actual values of the distances from N to the other mss. However, they do seem to yield acceptable results, and there is an encouraging amount of agreement between results due to these three "fragment" techniques.

It is reasonable to imagine that although our map assigns locations only to those extant mss which have been used in the analysis, every ms - whether extant or not - has somewhere on the map a place of its own, which we could estimate if we knew enough of its readings.

2. THE TEXTUAL TRADITIONS ANALYSED

The tradition with which this thesis is primarily concerned is that of the Syriac Psalter. However, I have also worked on various other textual traditions - biblical, classical and patristic. These studies were undertaken firstly in order to supply a wider background against which the potentialities of the methods here advocated might be judged, and secondly in order that I might benefit from the comments of experts in these fields of study adjacent to my own.

The six textual traditions were:

(a) A model tradition, invented by Dom Quentin in order to illustrate his own methods of analysis¹. Dom Quentin took a short passage (in Latin) as his original text, and constructed a genealogy involving 22 mss, some being conflate. He simulated the behaviour of the scribes at each copying, and presented an apparatus criticus giving the readings of all 22 mss. The text, genealogy and apparatus are reproduced in Thes., pp. 5:3 f.

It may seem strange that I should have turned to an artificial tradition, when there are so many real traditions worthy of analysis. But the Quentin model is special in that we know from the outset the entire history of the text, and so it can be used as a check on the validity of our

1. See his *Mémoire* (cf Thes., p. 1:5), pp. 213 ff. Dom Quentin's methods of textual criticism are considered in Thes., pp. 11:3 ff.

conclusions. For if we could never conduct a test on these lines, we could hardly ever be certain that inter-manuscript relationships suggested by MDS and by our interpretations thereof were indeed real, rather than only apparent and due to the method. Moreover, to have analysed by MDS a tradition whose history is known gives us a great deal of help when we first come to the question of interpreting ms maps.

(b) Cyprian: De ecclesiae catholicae unitate, edited by Prof. M. Bévenot¹. I have based my map on Bévenot's collations of the whole treatise, and on the eighteen mss fully reported throughout, viz a b B D e G h H J k m O p P R T W Y. The cent. v ms S, which is available only over the first few pages of the treatise, is also located, as a "fragment". For details of my policy in admitting variants, see pp. 2:40 f.

(c) Aeschylus: Persae, lines 1 - 746, after the collations of Dr. R. D. Dawe². The sixteen mss which are fully reported over that section were employed, viz A B C Δ H I K M N Nd O P Q V Y Ya. In addition to a map based on divergence measures of the type³ $d(A,B)$, a second map was obtained, on the basis of connection measures

1. "The Tradition of Manuscripts: a Study in the Transmission of St. Cyprian's Treatises", Oxford 1961.
2. "The Collation and Investigation of Manuscripts of Aeschylus", Cambridge 1964. Details of my policy regarding variants are given on p. 2:45.
3. Cf. Thes., p.3:4.

e(A,B) etc.¹ A comparison of the two maps is instructive.

(d) Extracts from the Vulgate Isaiah, according to the great Rome Vulgate now in progress². I undertook this map because I found that a table of distances had already been prepared by other workers, so that little "hack work" would be required apart from the computer processing. Thus I have worked not from the variants themselves but from the numerical results of others; it is obviously undesirable that one's acquaintance with the textual evidence should be so indirect. The map is based on the table entitled "lectiones discordantes" on p. xxx, which covers a sample of 219 readings; drawn from Isaiah Chap. 30; 31; 47; 49:1-6; 61:1-63:6. That sample does not include unique readings and purely orthographic variants (p. xxix). A selection of 28 mss is there considered³.

(e) Extracts from the Gospel of Luke, after the collations of J. G. Griffith⁴. Here again I have worked from tables of figures only. The text sample on which the map is based consists of three sections, viz 8:5^b-43;

1. On the meaning of this symbol, see p. 3:4.

2. See p. 3:4, n.6.

3. They are all shown in the map in Thes., p.6:54.

4. This material was used, but not presented in its entirety, in his article, "Numerical Taxonomy and some Primary Manuscripts of the Gospels" (J.T.S., 1969, pp. 389-406).

14:1-15:17; 23:1-40, and describes the 14 witnesses X, A, B, D, E, L, S, W, Θ, Π, Υ, ρ⁷⁵, fam 1, fam 13. A table of agreements for the first section is given on p. 398 of Griffith's article. For the other two, he generously put his own papers at my disposal. The sources from which Griffith himself drew his information are enumerated on pp. 394 f.

(f) The Peshitta Psalter, edited by W. E. Barnes¹.

All the variants in the whole book (within the limits laid down in Chap. 3) were used, and the map itself was drawn up for twenty-one witnesses. A number of fragmentary mss, and ecclesiastical writers who quote the Psalter, were also located thereon.

3. THE COMPUTATIONS

In order to derive the map and to carry out ancillary tasks such as the location of fragments, I have written a set of procedures, in Algol. They are suited to the machine which the University of London allowed me to use, viz the CDC 6600.

1. The mss considered are listed on pp. 7:6ff, and the ecclesiastical writers in Chap. 8.

The map programs were written directly from reference to Kruskal's papers¹. I found it necessary, in order to obtain each map, to submit two computer "jobs". In the first, ten independent random configurations were generated, and the iterative process improving the "fit" was allowed to go on for about one minute. The two configurations which at that stage gave the least stress were then selected, and in a second computer job iterations were continued (for up to about a further 100 seconds) until no further significant improvement could be achieved. In every case, the two converged to virtually the same map, which therefore merits confidence.

The maps are displayed at appropriate points in the thesis. Table B.4.1 gives the page number of these, and also the stress for analysis in three and in two dimensions. Only the two-dimensional maps are actually shown, in accordance with what was said above (p. 4:5), except that

1. Kruskal's articles are clearly written, and an experienced programmer will readily derive a computer program. One point may perhaps be elaborated. On p. 125, Kruskal speaks of "the (negative) gradient" and gives the formula for a quantity g_{kl} ; but g_{kl} seems to be the gradient itself. Thus, using g_{kl} the notation of p. 120, we have to decrease x_{is} , at each iteration, by $\alpha g_{is}/\text{mag}(g)$, not increase it (as Kruskal's equations on p. 120 suggest). For help in "de-bugging" my program, I am indebted to the advisory staff at the University of London Computer Centre, in particular to Dr. John Packer and Miss Jenny Rooke.

one three-dimensional map, of the Cyprian data, is presented (fig. B.4.1.) for the purpose of illustration.

TABLE B.4.1

<u>Textual Tradition</u>	<u>Number of manuscripts</u>	<u>Page No.</u>	<u>Stress (%) For map in three / two dimensions</u>	
Quentin model ¹	19	5:7	3.676	7.534
Cyprian: De Unitate	18	6:4	7.703	10.830
Aeschylus: Persae (divergence measures)	16	6:40	5.970	13.681
ibid. (connection measures) ²	16	6:50	-	9.786
Vulgate Isaiah	28	6:54	11.137	16.031
Gospel of Luke ³	14	6:60	9.190	10.298
Peshitta Psalter	21	7:9	8.352	10.980

These values for the stress are encouraging, for they are of indubitable statistical significance, as can be appreciated in the light of D. Klahr's study⁴: each value is well below the 5% percentile point determined by Klahr for the appropriate number of points and dimensions.⁵

The location of fragments required a procedure for minimising the quantities S_1 and S_2 defined above. This too was written in Algol⁶; it is based on a paper by J. A. Nelder and R. Mead⁷. To locate a new ms by using this procedure usually took only five seconds or even less.

1. There are in fact 22 mss, but if we discount unique readings, three of them (MTZ) are indistinguishable from their respective exemplars (LSY) and were not therefore included in the analysis.
2. As an experiment, both types of measure were employed in connection with the Aeschylus data (p. 4:13). For the second, a three-dimensional map was not attempted.
3. The device mentioned above (p. 4:7) was necessary in order to avoid a trivial solution.
4. "A Monte Carlo investigation of the statistical significance of Kruskal's Nonmetric Scaling Procedure", in Psychometrika (1969), pp. 319-330.
5. Selected percentile points (none lower than 5%) are tabulated on p. 328. Those that concern us here may be extracted:

	Nc. of points in map	12	16
Percentile point for	{ 2 dimensions	21.1	25.7
	{ 3 dimensions	11.8	17.0

For any given number of dimensions, of course, the 5% percentile point will rise if the number of points in the map is increased.

- 6! Mr. A. Orr, of University College London, gave me a copy of a Fortran subroutine, which I then translated - with the aid of Mrs. J. Farmer of the Northern Polytechnic, who taught me the elements of Fortran - into my "native" Algol.
7. "A simplex method for function minimisation", Computing Journal vii (1965), pp. 308-313.

Explanation. Each ms is to be thought of as standing at the top of a "tower" or the bottom of a "mine-shaft", whose height (or depth) is represented by an appropriately directed arrow (drawn to scale) which starts out from "ground-level". Just how effectively this representation conveys the impression of three dimensions, is debatable; a different method of presenting a 3-D map (for the Aeschylus data) is illustrated on p. 6:47.

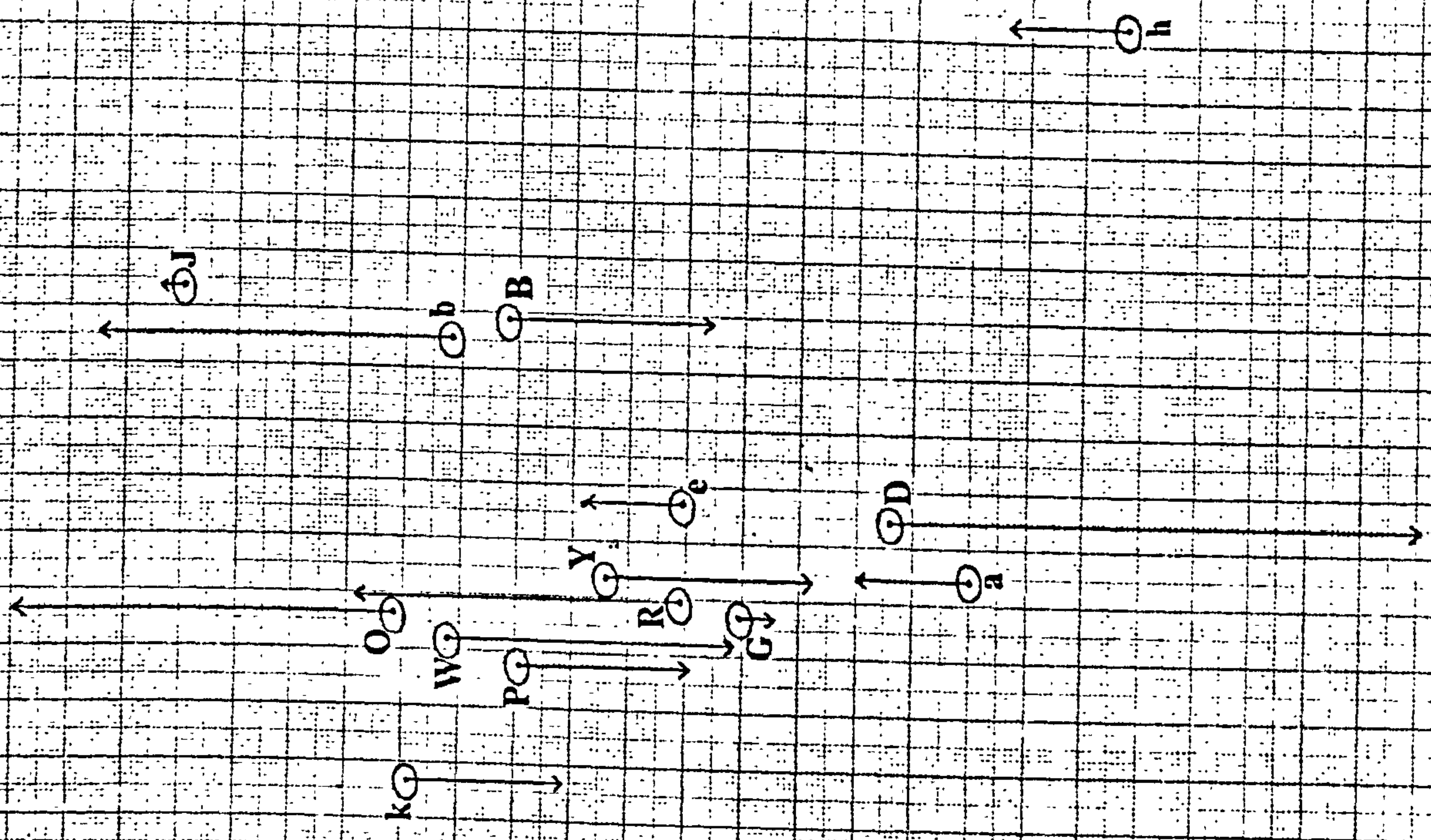


FIG. B.4.1.

4. OUR DATA AND REPRESENTATIONS THEREOF IN THE LIGHT OF INFORMATION THEORY

It is possible to estimate numerically the information content - in the sense of C. E. Shannon¹ - of the textual data, and the maximum amount of information that can be conveyed by means of different systems of representation (clusterings, spectra, maps).

Let m be the number of mss collated, and let V be the number of variant passages considered. What is the information content of the resulting list of splits?

Let us assume, to simplify matters, that all the splits are two-way. This is not, of course, usually true, although the great majority of the splits do tend - within my experience - to be two-way. The total number of different two-way splits into which m mss may be divided is $2^m - 1$, and so the number of bits of information conveyed when we are told that in any given passage the mss divide themselves in a particular manner, is approximately m . (If one of the m mss is defective at any given point, then one bit of information is lost.) Thus the information over the whole domain is in the region of mV bits. For example, my data on the Peshitta Psalter, covering 21 witnesses (some of which have serious gaps) over 447 variant passages, must embody about 8000 bits of information.

1. "The Mathematical Theory of Communication", by Claude E. Shannon and Warren Weaver, Urbana 1949.

The amount of information which is associated with the various representations may be estimated as follows.

Suppose that we decide to employ one particular representation, say a spectrum; and let $S(m)$ be the total number of different spectra that can be made up from m mss. Then, when we specify that spectrum which we believe to be appropriate in the light of our data, we are stating that of all the $S(m)$ possibilities, this particular spectrum has been selected. The information thereby conveyed¹ is $\log_2 [S(m)]$ bits. And we may reason similarly about each of the other possible representations.

Let us therefore calculate the total number of different clusterings, maps etc. which can be made up for m mss. The logarithm to the base 2 of that number will give the maximum amount of information that can be conveyed by our selecting one particular clustering, map, and so on. These quantities have been calculated in Table B.4.2, for $m = 5, 10, 15, 20, 30, 40$. The reasoning behind those calculations will now be given.

1. This is a convenient expression, albeit somewhat misleading. We are measuring here, not the information which is imparted by any particular spectrum, but the amount of information which we associate with a choice between $S(m)$ possible spectra.

Cluster analysis will generally yield a binary tree. If $B(m)$ denotes the number of possible binary trees for m extant mss, then it is easy to shew, by considering the different ways in which a binary tree of k mss can be augmented by another ms to yield a binary tree of $(k + 1)$ mss, that

$$B(k + 1) = (2k - 1) B(k),$$

whence $B(m) = (2m - 3)!!$

which is the number of possible clusterings of m mss.

We now come to the number of different spectra. There are $m!$ different orders in which m mss can be arranged. However, every ordering has the same force as the reverse ordering; for example, the two spectra ACBD and DBCA are equivalent. The number of truly different spectra is therefore $(m!)/2$.

In order to calculate the number of possible maps, we recall that the map is generated from the rank ordering of the $\frac{1}{2}m(m-1)$ data distances. The number of different possible orderings of these is, of course, $\{\frac{1}{2}m(m-1)\}!$ If we assume that (a) each of these possible orderings is conceivable (i.e. it is capable of being produced by an appropriate collection of splits), and (b) no two different orderings lead to an identical map, then $\{\frac{1}{2}m(m-1)\}!$ is also the number of possible maps. I have not proved either assumption, but it seems intuitively likely that (a) is true, because we have at our disposal $2^{m-1} - 1$ different types of split, the numbers of each of which can be varied, in order to satisfy only the $\frac{1}{2}m(m-1)$ inequalities concerned. As for

(b), this does not always seem to be true, at least in cases wherein m is small and a map with zero stress can be obtained¹. Even so, it seems that although $\{\frac{1}{2}m(m-1)\}!$ may be an over-estimate for the total number of possible maps, it is of the right order of magnitude.

Another representation, which we need not consider at length, is the general (as opposed to binary) tree. This is not generated, to my knowledge, by any of the methods usually employed for the analysis of such data². The total number $T(m)$ of possible trees may be calculated as follows. Let $\tau(m,i)$ represent the total number of possible trees containing m extant mss and i lost point mss. Then it is not difficult to shew that

$$\tau(m+1,i) = (m+i-1)\tau(m,i-1) + 2(m+i)\tau(m,i) + (i+1)\tau(m,i+1).$$

Given that $\tau(1,j)=0$ for $j \neq 0$, $\tau(1,0)=1$, we may successively build up the terms τ to any value we please, within the limits of our computation facilities. Then $T(m) = \sum_i \tau(m,i)$. There does not seem to be any simple analytical expression³ for $T(m)$.

-
1. Consider the following counter-example, where we have three extant mss ABC. We may imagine the two following different rank orderings of the inter-ms distances (here AB denotes the distance $d(A,B)$), viz (i) $AB < AC < BC$, (ii) $AB < BC < AC$. If we accept trivial solutions, both will yield a map on which AB are represented by a single point and C by another. Even if we add a hypothetical ms M (in accordance with p. 4:7) such that $AM=BM=CM < AB$, we can still construct a two-dimensional map that gives zero stress for either (i) or (ii): it has $AB=2$, $AC=BC=3$, $AM=BM=CM=9/4\sqrt{2}$.
 2. except, of course, by orthodox stemmatics; but we are dealing in this Chapter with traditions for which no satisfactory stemma can be obtained.
 3. Maas (pp. 27 ff.) shews that $T(3)=22$, and estimates (see p. 29) $T(4)$ as 250 and $T(5)$ as 4000. W. Hering (Philologus 1967, p. 175) gives $T(4)=262$, $T(5)=4336$, $T(6)=91984$; my calculations agree with Hering against Maas. Since $\tau(m,i)=0$ for $i > m$, it may be shewn that:

$$T(m) \leq \prod_{i=1}^{m-1} (7i - 4).$$

We now shew, in Table B.4.2, the maximum amount of information to be conveyed by the representations available.

Representation	TABLE B.4.2.					
	Maximum amount of information (in bits, to the nearest integer) for the following values of m:					
	<u>5</u>	<u>10</u>	<u>15</u>	<u>20</u>	<u>30</u>	<u>40</u>
binary tree	7	25	48	73	129	189
spectrum	6	21	39	60	107	158
map	22	186	558	1169	3191	6375

It will be seen that the map has by far the most information - in the technical sense - and that its advantage increases rapidly with m. Looking at it from another point of view we may say that we have far more freedom of choice if we employ a map than for other representations; and this means that, other things being equal, the method of mapping offers us the best chance of providing an appropriate description of the data.

We note that even in the case of the map, the maximum amount of information that can be conveyed may fall short of the information content of the data. Thus, as already mentioned, the data for the Peshitta Psalter contained about 8000 bits, whereas the map (of 21 witnesses) could not convey more than about 1300 bits, approximately one-sixth of the information given. Even so, I am impressed by the sensitivity of the mapping method, as the following incident will illustrate. When I first obtained by computer the distance matrix for the

Peshitta Psalter, 447 data cards were submitted of which each recorded a single variant passage. Without my knowledge, the card reader accidentally skipped one of the cards. On the basis of the percentage distances thus obtained, a map was generated. Subsequently I detected the omission, and had the percentage distances re-computed. It turned out that the rank ordering of the distances differed from that which had been obtained before; and when the map was re-submitted so that it could be seen whether any adjustment was now required, Kruskal's procedures showed that not inconsiderable adjustments were indeed necessary in order to reach once more a "minimum stress" configuration, and the new map was noticeably, albeit slightly, different from the earlier one. Thus the replacement of this one card did have some effect on the map.

The reduction of the data to the form of a map will entail, then, a substantial loss of information¹. However, as we remarked above (p. 3:9)², we shall be well compensated if the map can be interpreted so as to bring order out of the near-chaos of the data.

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1. This expression deserves to be amplified. In order to specify completely the ways in which the mss group themselves at each point in the Peshitta Psalter, we would need about 8000 bits. To specify which out of all the possible maps has been selected to describe that data would take, however, only about 1300 bits. It is in this sense that the term "loss of information" is intended.
 2. in connection with the information loss (in a less technical sense) due to the reduction of the list of splits to inter-manuscript distances.

5. The Interpretation of the Map

Having gone to the trouble of constructing a map, we must ask ourselves two questions. First, what does the map tell us that we could not have discovered from our textual data in its "raw" state? And in particular, how far does it go in supplying the desiderata outlined in Chapter 3?

At first I could give no satisfactory answer to these questions. True, the map was of immediate interest from several points of view. There are obvious advantages in representing the similarity relations of the mss in a pictorial form, which can readily be comprehended. Thus the literary scholars to whom I showed maps of textual traditions with which they were themselves familiar, felt that the map was meaningful; but this is far from having a detailed interpretation in terms of textual history or critical policy. Again, I found that when the map allowed definite groups (or "clusters") of mss to be identified, these conclusions usually agreed with those reached by other investigators on purely "literary" grounds. This was comforting; it could be claimed that whereas a great deal of painstaking labour had hitherto been required, in most cases, in order to identify the ms families, all that would now be necessary would be to present the ms readings to the computer, which could then be left to group the mss far more rapidly and with less risk of subjectivity than before. But if that is all the map can offer, then we may well ask ourselves whether the results justify the effort.

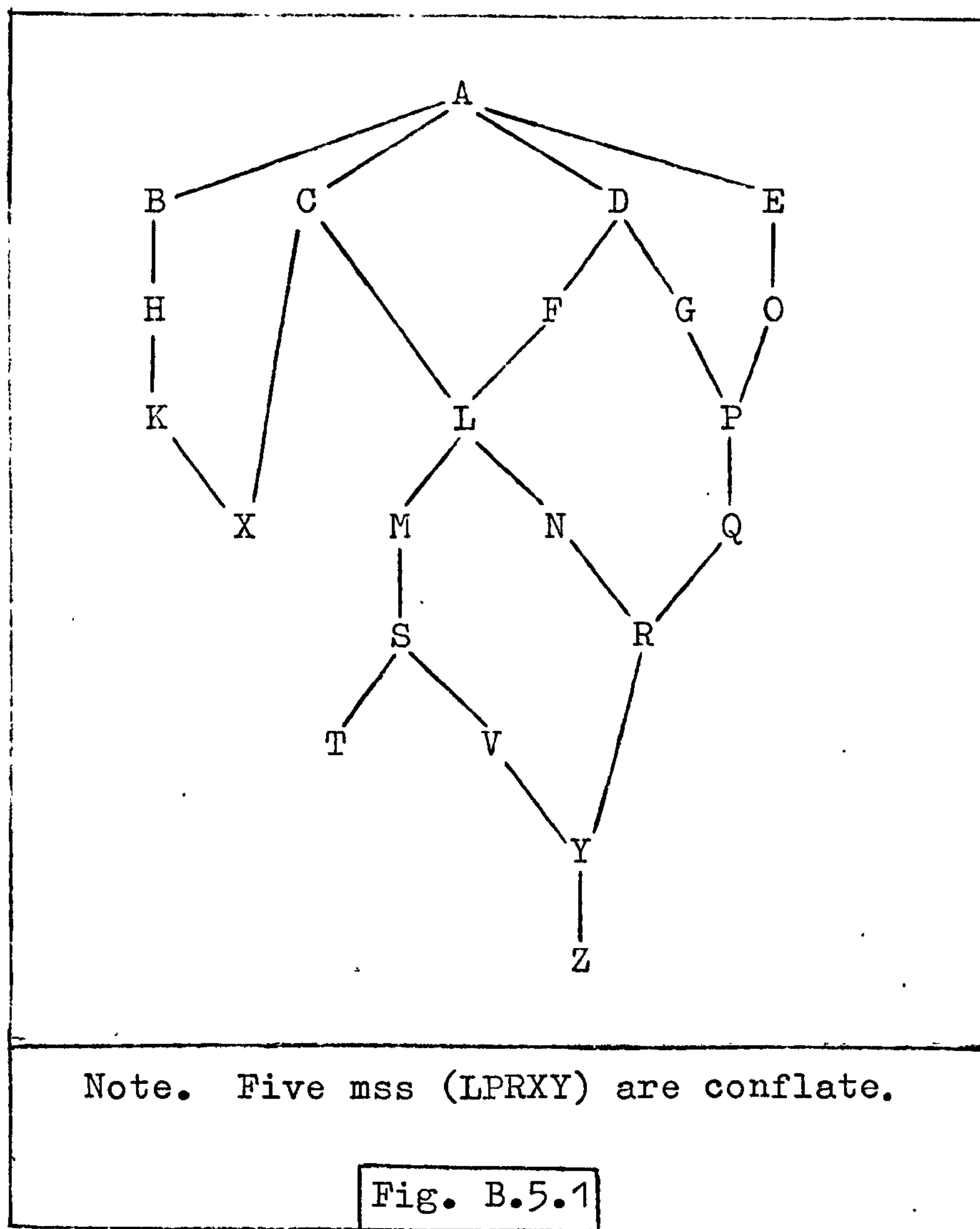
Perhaps the most usual way of interpreting a map derived by MDS is to extract meaning from the co-ordinates. In most of my maps, I have not been able to explain the ms positions in such a way (e.g. one might have stated that the two co-ordinates of a ms represent the degrees to which it bears the marks of corruption and interpolation respectively). Admittedly, in the case of the Peshitta Psalter it turned out that the x-co-ordinate could be interpreted geographically, while the y-co-ordinate seemed to correspond to the dates of the mss¹, and thus it is possible that other maps will also be found to be interpretable in terms of space and time. But however interesting such an interpretation might be, it would not in itself be very useful for the purpose of constituting the text.

In my search for a more satisfactory approach to the problem of interpretation, there seemed to be only one thing left to try: to consider the map of a textual tradition whose history is known, so that we have all the facts from the beginning. Such a tradition was invented by Dom Quentin,² who took a paragraph of a dozen lines, in Latin, and invented a history of its text. The correct text is called A, and copies (B-Z) are made to give 22 mss in all.

1. See p. 7:10. Mss written in the East tended to come out on the right of the page, and those from the West on the left. Early mss were found mainly towards the bottom of the map, and late mss towards the top.

2. "Mémoire sur l'établissement du texte de la Vulgate", Rome and Paris 1922, pp. 213 ff.

Let us therefore consider in detail Quentin's experiment and the map to which it leads. The original text is given on p. 5:4. From this text (A), others are derived by the following genealogy:



Each of the 21 copies diverges from A. The details of the readings introduced into each ms are given by Quentin (pp. 214 ff.). Thus we may now present the text of A, with an apparatus criticus:

DOM QUENTIN'S MODEL TRADITION

THE ORIGINAL TEXT

- 1 Anastasia primo diram et inmitem custodiam a viro suo Publico passa est,
 2 in qua tamen a Chrysogono, confessore Christi, multum consolata et confortata
 3 est. Deinde a praefecto Illyrici in gravissima aequae et diutina custodia macerata
 4 est: in qua duobus mensibus refecta est caelestibus escis per sanctam
 5 Theodoten, quae prius martyrium passa est. Deinde navi imposita cum ducentis
 6 viris et septingentis feminis, ut demergerentur in mari, perlata est ad insulas Palmarias
 7 ubi martyrium consummavit: et omnes qui cum illa venerant
 8 variis interfectionibus martyrium celebrarunt.
 9 Inter quos omnes, unus erat nomine Eutychianus,
 10 innocentissimae naturae, qui sublati sibi, cum dives
 11 esset, omnibus facultatibus, tacuit, nihil cogitans nihilque
 12 metuens, nisi hoc, ne facultates ac divitias fidei
 13 perderet. Quotiescumque denique fuisset auditus, quotiescumque
 14 interrogatus nihil aliud dicebat: Christum
 15 mihi non tollet etiam qui caput abstulerit.

THE APPARATUS CRITICUS

- 1 duram STVYZ suo) eius N; om. DFG Publicio VYZ
 2 Christi) om. CLMNSTV consolata) add. est BHKX
 3 est) om. BHKX deinde) dein FLMNRSTVYZ; dehinc KX Illyrico CX
 gravissimam KX aequae et) atque DFGLMNPQRSTVYZ
 diutina) diutinam KX; diuturna ECPQRYZ custodiam KX
 4 mensis VYZ caelestibus escis) om. N sanctam) beatam Z*
 5 Theodotem BHKX; Theodotionem N prior DFGLMNRSTVYZ
 passa) perpessa CLMNSTVXYZ navim OPQRYZ trecentis GPQRYZ
 6 septingentis) septuaginta CLMNSTVX; om. O
 demergerentur) demergeretur BHK; mergerentur QRYZ
 mare EOPQR insulam Palmariam DFG
 7 consummarunt FLMNRSTVYZ quae NRYZ illa) ea GPQR venerunt CLMNRSTVXYZ
 8 martyria STVYZ
 9 omnes) om. DFGLMNSTV erat) fuit BHKX nomine) cui nomen YZ; om. R
 Eutychius OPQRYZ
 10 innocentissimus CLMNSTV natura LMNSTV sublatas FLMNRSTVYZ
 dives) locuples Z
 11 omnibus facultatibus) add. suis DGPQ; omnes facultates suas FLMNRSTVYZ
 tacuit) sustinuit FLMNRSTVYZ nihil enim cogitans STVYZ
 12 metuens) dicens CLMNRSTVXYZ fide T
 13 perderet) non perderet CLMNRSTV; perderentur OPQ*
 denique) enim DFGLMNPQRSTVYZ fuisset) fuit BHKX; om. O quoties GPQ
 14 interrogatur T dixit BHKX dicebat) add. nisi DFGLMNPQRSTVYZ
 Christum) Iesum Z
 15 tollet) tolles HKX; auferet NR qui) si HKX
 abstulerit) abstuleris HKX; absciderit QRYZ

* In Quentin's own apparatus (pp. 217 f.), the report of the mss at this point seems to have been misprinted.

A certain difficulty is encountered before we can derive the map. One of the mss (M) is identical throughout with its exemplar; and if - in accordance with our usual policy - we neglect unique readings, then the same is true of two other mss (TZ). It therefore seemed best to omit these three mss. As for the other nineteen, the distance measures between each pair were calculated, and then the map on p. 5:7 was derived.

Let us look at this map in the light of our knowledge of the textual history. There are, of course, a great many questions to ask. Let us first consider those mss which derive from only one source. How is each of them located in relation to its exemplar?

We may take, as an example, ms E with its descendant O. It will be seen that O lies more or less along the line leading from the original (A) to E, being some way further out than its ancestor. In other words, the angle $\angle AEO$ is about 180° . Again, although the path A-B-H-K is not perfectly straight, nevertheless the angles subtended along its intermediate points ($\angle ABH$, $\angle BHK$) are not far from 180° . The reader may verify this for all those mss which have only one ancestor (FGHKNOQSV). As we have already noted, however, the paths are not absolutely straight; we shall return to this presently.

These observations are not really surprising. We have seen that, on the map, E lies more or less exactly between the original and its own direct descendant. This corresponds to the fact, that E is indeed the link, the sole intermediary¹,

1. The term is, of course, "Quentinian"; but the general approach to the problem suggested in this thesis is, as the reader will appreciate, very different from that of Dom Quentin.

between A and O. Thus, a copy made from a single exemplar will be in the same general area of the map as its exemplar but at a greater distance from the original - which is just what we might have expected.

Why then are the paths in question not perfectly straight? One reason will be appreciated if we consider D, from which were made two independent copies, F and G, which disagree with each other in 8 passages. If both ADF and ADG were straight lines, then the path AFG (or AGF) would also be a straight line; and thus it would seem as if G were derived from F (or vice versa), while we would lose sight of the divergence between F and G. The map compromises, as it were, by placing F and G on opposite sides of AD produced. Thus the paths ADF and ADG are bent away from each other, so to speak, and the mutual independence of F and G is respected.

We now consider the location of those mss which are of composite origin. Such a ms is X, which is conflated from C and K. A line drawn from A to X runs between the lines AC and AK. In other words, the location of X is something of a compromise between that of a descendant of C alone on the one hand, and that of a descendant of K alone on the other. We may similarly expect the location of a ms derived from three exemplars to be a suitable compromise in an analogous fashion, having regard to the respective extents to which the scribe who made the copy drew on his three sources.

The reader will verify that the bearing from A of each of the other four composite mss in the tradition (LPRY) is consistent with what we have said of X. We may ask moreover

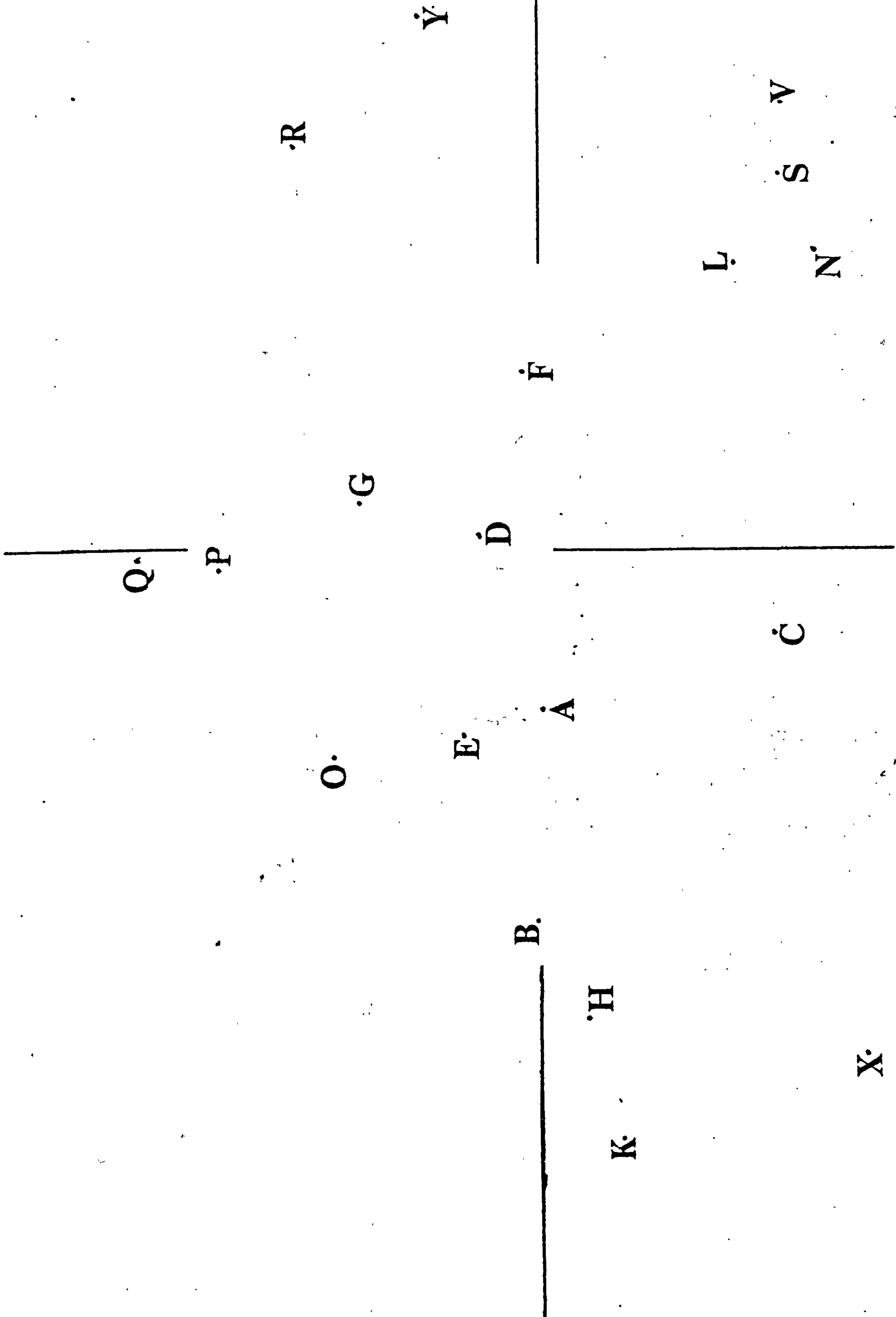


Fig. B.5.2

how far away from A a composite ms will appear on the map. This will depend on the judgment of the scribe in his use of his different sources. The five hypothetical scribes in this case did not take full advantage of the opportunity of comparing two exemplars; they took over many errors from both sources, so that each of these five copies is more heavily laden with error than either of its exemplars. Accordingly, X is further out (from A) than either C or K. However, we surmise that many scribes made more discriminating use of two exemplars and "at times the checking of one manuscript against another produced a better text than either of them".¹ Such a ms would presumably be closer to A. Thus if a more learned scribe, say U, had made another copy from C and K, we might find U about midway between A and X.

If we were given this map, together with the information that A is the original, we would now be able to recover, to a considerable extent, the textual history - or rather, if we bear in mind the axiom of equivalence, a textual history compatible with the textual data. But in real traditions, we are not so fortunate, in that a very great number of mss - many times more numerous than the extant mss, and including the original itself - are lost and as yet not located on the map. We cannot therefore deduce, without further ado, a textual history to serve as a basis for critical rules. Instead, it seems best to consider in greater detail the properties of the map itself.

1. M. Bévenot's paper of 1970 (see Thes., p.71:113), p.3.

We may ask whether the map allows us to discern any order in the various groupings evinced by the mss at different points in the text. It turns out that in each of the fifty-odd variant passages, the mss group themselves in such a way as to divide the map into "simple", or continuous, regions. This is illustrated by the examples of fig. B.5.3.

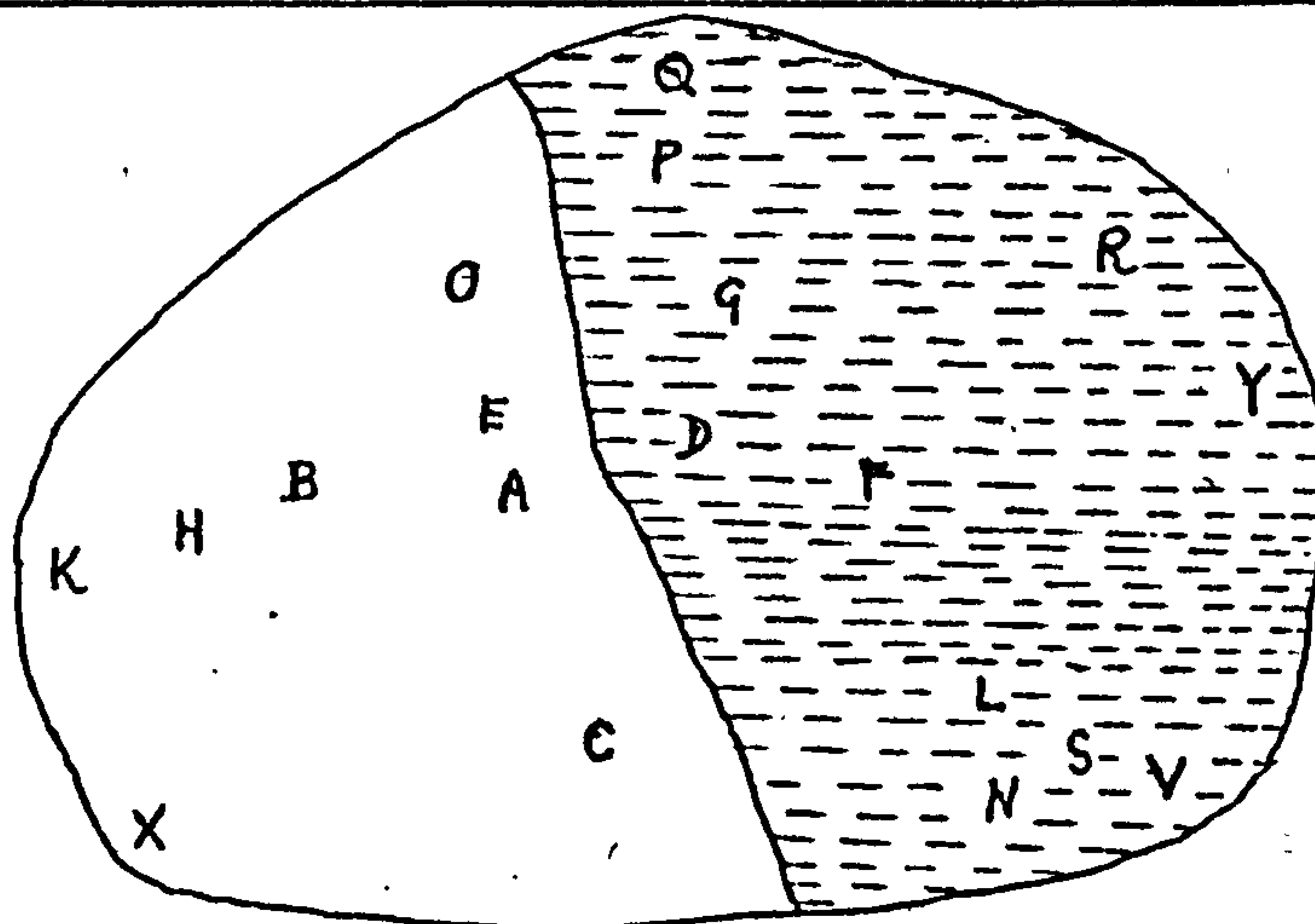


Fig. B.5.3(a)

UNSHADED AREA
aeque et (1.3)

SHADED AREA
atque

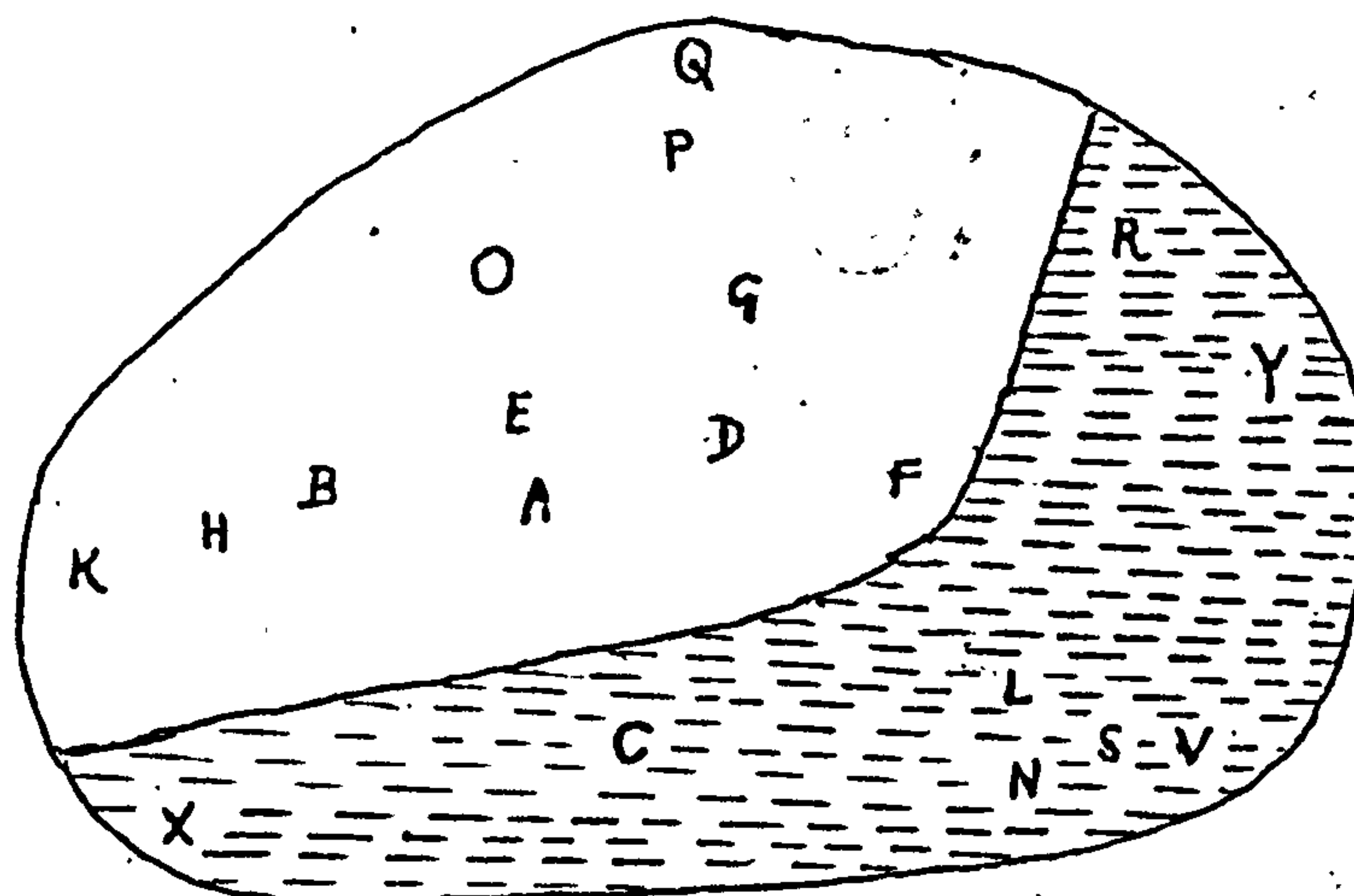


Fig. B.5.3(b)

UNSHADED AREA
venerant (1.7)

SHADED AREA
venerunt

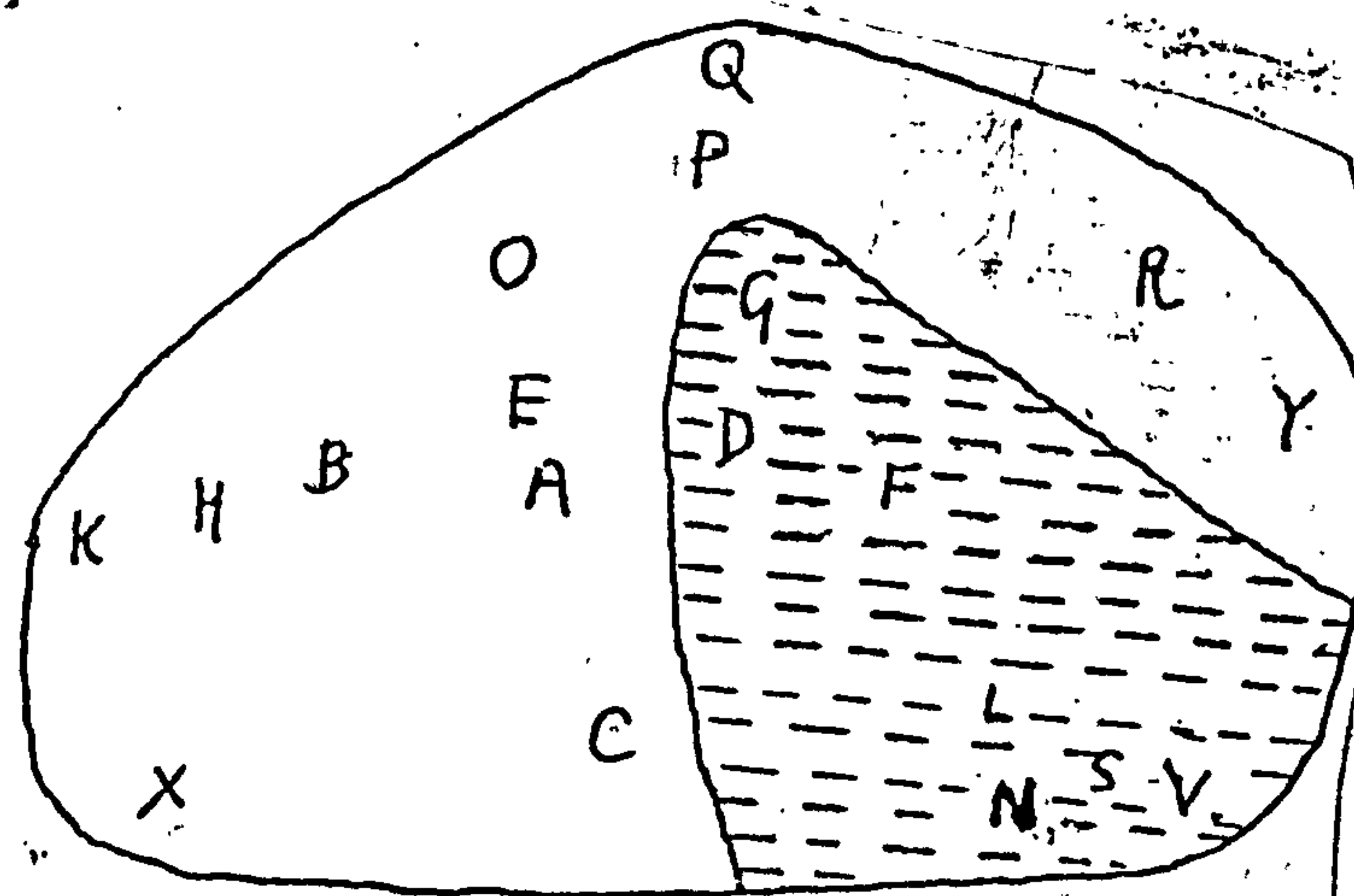


Fig. B.5.3(c)

UNSHADED AREA
omnes (1.9)

SHADED AREA
omnes is omitted

Note. This diagram illustrates the text of p. 5:11.

Lest it be thought that virtually any grouping of the mss will yield "simple" regions, I have generated two random splits of the 19 mss. The first was devised to group the mss nine against ten, and yielded the division Σ :BCGHNOQVX. The second was a split of the mss six against thirteen, viz Σ :BCKNQS. Both were obtained by the use of a table of random numbers. It will be seen from fig. B.5.4. that the regions into which the map is now divided are of quite an irregular shape. The difference between a "simple" and an "irregular" region is one which I cannot define mathematically: but we can appreciate intuitively that its existence is real. This difference seems to be one of degree rather than of kind.

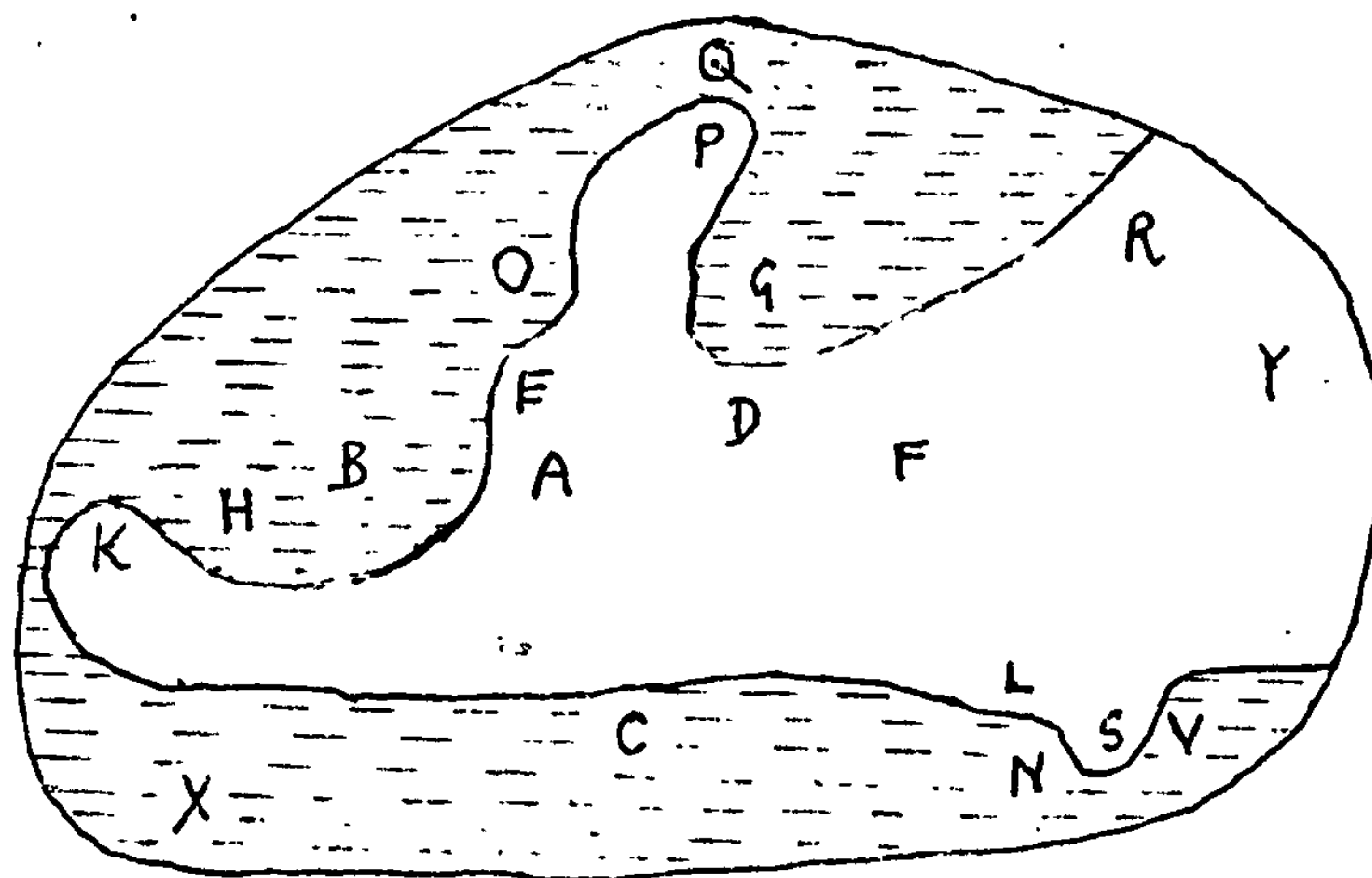


Fig. B.5.4(a)

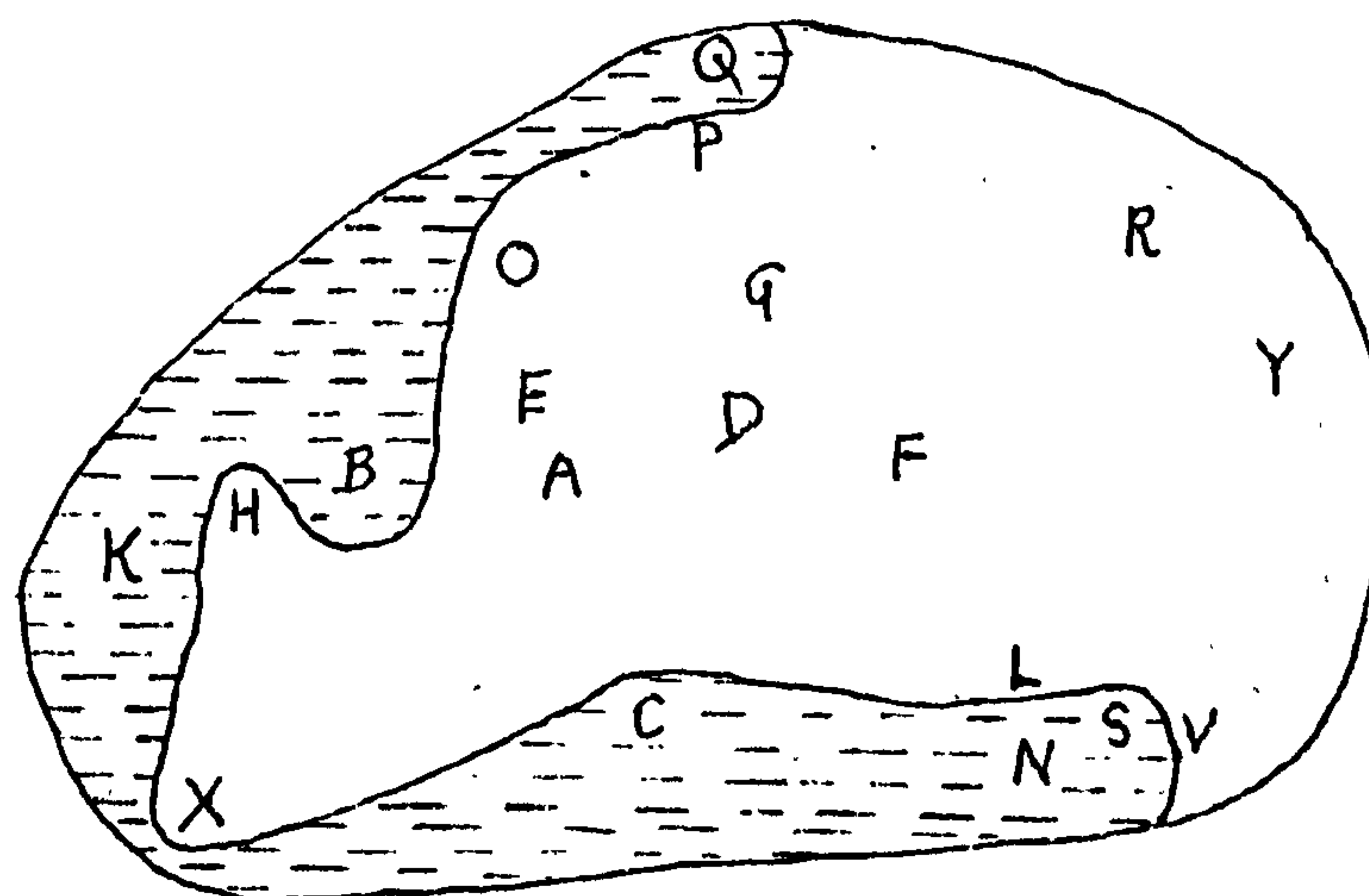


Fig. B.5.4(b)

Note. In both (a) and (b), the region containing less mss is shaded. For either diagram, there are other possible ways of drawing a curved line to separate the two ms groups, but none of them seems to give regions of an appreciably more regular shape.

This phenomenon of continuity is exhibited not only in this model tradition but in real traditions too, as will be seen in Chapter 6. It may be explained in terms of what was said above on the way in which the map represents ms relationships. For example: the word omnes (1.9) was omitted in D, and therefore also in FG. Now L had available two sources, viz C, which had omnes, and F, which did not; he preferred F here, so that the error spread to LMNSTV. Like L, P was also faced with one ms having the correct reading (O) and one ms marred by the omission; but P made the right choice, so that the omission did not infect PQ. Similarly, R and Y each consulted two exemplars of which only one included "omnes", and both R and Y chose - correctly - to retain the word. To sum up: the error arose in a certain ms, at a certain point on the map; thence it spread successively to different mss. It was adopted by any scribe who was prepared to adopt the erroneous reading which he found in his exemplar (or in one of his exemplars); its progress was checked whenever a scribe succeeded, by consulting another source or - though that is not the case here - by conjecture, in restoring the true reading.¹ Thus the process whereby the erroneous reading

1. Further illustration of the multifarious possibilities is provided by the work of pp. 2:37f. Let us regard fig. B.2.10 as a tree in which each ms is joined to its main exemplar but may also have had subsidiary sources. On p. 2:38 are shown some of the ms groupings which result when scribes correct away an error found in a main source and thus check its diffusion. The fact that the set of scribes who succeeded in so doing varies from one passage to another, gives rise to a great variety of ms groupings; these frequently fail to satisfy the condition of mutual stemmatic consistency, but may be easily represented on the map as continuous regions.

comes to occupy a region of the map may be compared to the spreading of an unpredictable ink-blot.¹ The area which the error will cover in any given case will therefore be a continuous one.

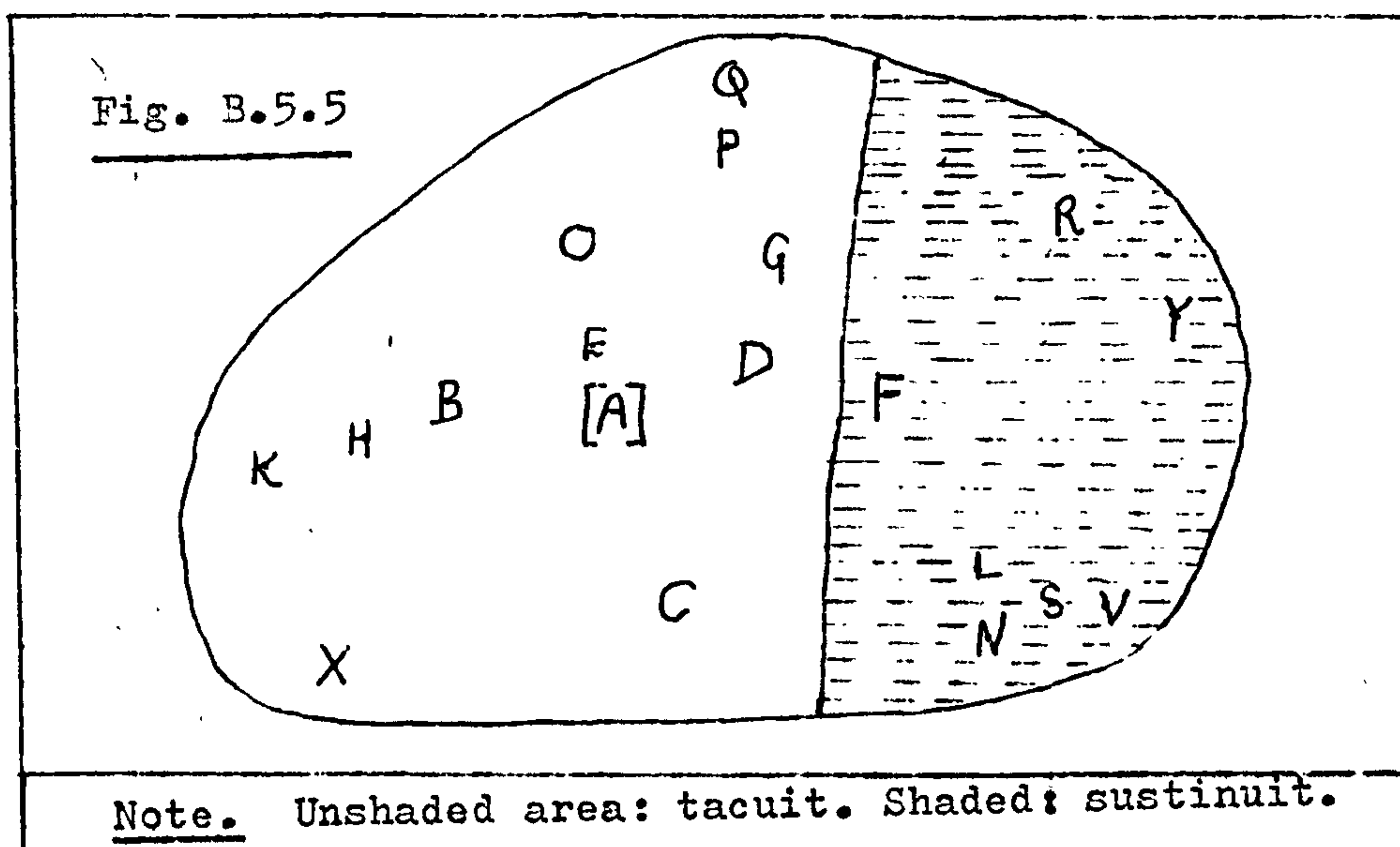
Following this simile, we shall find it convenient to use the term "smear" with reference to the diffusion of an error over part of the map. Thus we may speak of the smear made by a given erroneous reading.

We note that if the same error originates independently in two mss, then two smears will be produced, and that reading will be represented in two different regions of the map.²

The importance of the continuity property of the map lies in the fact that we may be thereby enabled to estimate the reading of a lost ms - and, in particular, the lost original - if we can estimate where on the map that lost ms would have lain. Suppose, for example, that A were lost,

1. Indeed, Pasquali has already used such a simile to describe the transmission of a variant reading (p. 141): "...la trasmissione orizzontale, se non di un testo, di singole lezioni si confronta molto meglio con una macchia d'olio che da un punto determinato si allarga a poco a poco sino a coprire tutta una superficie: fino dove giungerà allargandosi, nessuno può prevedere con sicurezza". The map now provides us with a surface over which this spreading can be traced.
2. There is a danger of two such smears being produced fairly close to each other on the map and thus being mistaken by the investigator for a single region. On this question see p. 5:20 below.

but that its location on the map were known.¹ Consider now the variant in l. 11, where ten of our mss have sustinuit, whereas the remaining eleven (BCDEGHKOPQX) have tacuit. Which is the reading of A, the lost original? It will be seen from fig. B.5.5 that the map divides into two regions, and A falls without much doubt



into that occupied by tacuit. The continuity principle makes it likely that the reading of A was that of all the other mss in the surrounding area, viz tacuit.

This immediately raises some important questions. First, how are we to estimate the location, on the map, of the lost original? Second, how exactly are we to carry out this process of estimating the original reading² in less

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1. We shall shew presently how the location of a lost original can be estimated.
 2. We assume for the sake of simplicity that there is only one original reading in each passage. If we suspected a multiple original, a map could of course be constructed, but we could not simply apply to it the theory of interpretation advanced here.

straightforward cases? For, the grouping of the mss and hence the division of the map into regions may be such that we cannot be sure which region would have included the point where we locate the original. Third, what degree of likelihood attaches to an estimate of the original reading obtained in this way?

We begin with the problem of locating the lost original, Ω ¹. In order to place Ω , we have to know - or rather to proceed as if we did know - some of its readings. That is, we have to find a number of passages wherein we can be reasonably sure which reading is the original one and was therefore in Ω . On the basis of these readings, we may now locate Ω as a fragment, after the methods described above (pp. 4:8 ff.)

The idea of adding to the map a point to represent a "best" text finds a parallel in the remote field of market research. P. E. Green and F. J. Carmone describe² how, having formulated a MDS map of various products that are actually available, one may locate thereon the "ideal point" of any individual who gives details of his own preferences. The ideal point will represent a hypothetical product possessing that combination of features which he most prefers; and he will tend to prefer a product whose map position is closer to his ideal point, rather than one which is further away. It is noteworthy that our policy of superimposing Ω on a pre-constructed map of the actual mss, rather than incorporating this ideal entity as just another specimen from the beginning of the analysis, is also paralleled in the work of Green and Carmone (p. 78).

Objections may be raised against this procedure on two counts, of subjectivity and of circularity; for we have to make a number of decisions as to the rightness or wrongness of readings before we can go any further. To the former charge I would reply that some subjectivity is inevitable. It will be recalled that in traditions which admit the construction of a stemma, the objective evidence will give us no more than an unorientated network; in order to determine the stemma and hence to obtain critical rules, we have to identify which ms group is in error in certain

1. In some parts of the Thesis (e.g. Chap. 7), I refer to the point at which Ω is located as the "best point".
2. "Multidimensional Scaling and Related Techniques in Marketing Analysis", Boston 1970; see especially pp. 4 ff, 11 ff and Chapter 4.

passages. It seems inconceivable that, in order for distributional inferences to be possible, one should require subjective judgments in "stemmatic" traditions, but not in more complicated traditions. We cannot then hope to escape the need for some subjectivity in choosing that point of the map where we shall locate the original, just as we required it in choosing that point of the network which would represent the archetype.

The introduction of subjectivity in the construction of the stemma led many scholars to bring the charge of circularity. An answer to this has already been offered on p. 1:44, namely that all is well as long as we do not later invoke our stemma in order to defend our earlier decisions (which will normally relate to a small number of passages only) between rival readings. This answer would apply here too if it were possible to locate Ω by means of a number of passages forming only a small proportion of the total. However, that cannot always be accomplished satisfactorily, as we shall see below; and so we must control the circular element as best we can, without always being confident of success.

One way is to work with two domains, as follows. The investigator is likely to find, that, in choosing the best

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1. I speak here of "original" rather than "archetype", because in a contaminated tradition little interest will attach to the reconstruction of the text of the latter (even though the concept of an archetype is not without value - p. A:66).

of the alternative readings offered by the mss, he can be far more confident in some passages than in others. There will be some in which he is virtually certain; others in which he has a strong preference for a particular reading but is not so sure that he would wager thereon a sum of money which he would find it inconvenient to lose; still others, wherein he can find no cogent grounds for preferring any one of the variants. We may first take the passages of the first category as a domain. Assuming that we have correctly identified the readings of Ω therein, we calculate the distance between Ω and each of the extant mss, and hence obtain a location for Ω . We may then define a second domain, to consist of all passages falling into either of the first two categories, and on that basis we locate Ω once more. If these two locations agree, our confidence in the result is enhanced.¹ An additional check is possible in cases wherein the dimensions of the map can be interpreted in terms of space and time; if we know the place and date of the origin of the book, this too will give us some idea of where we might expect to find Ω .

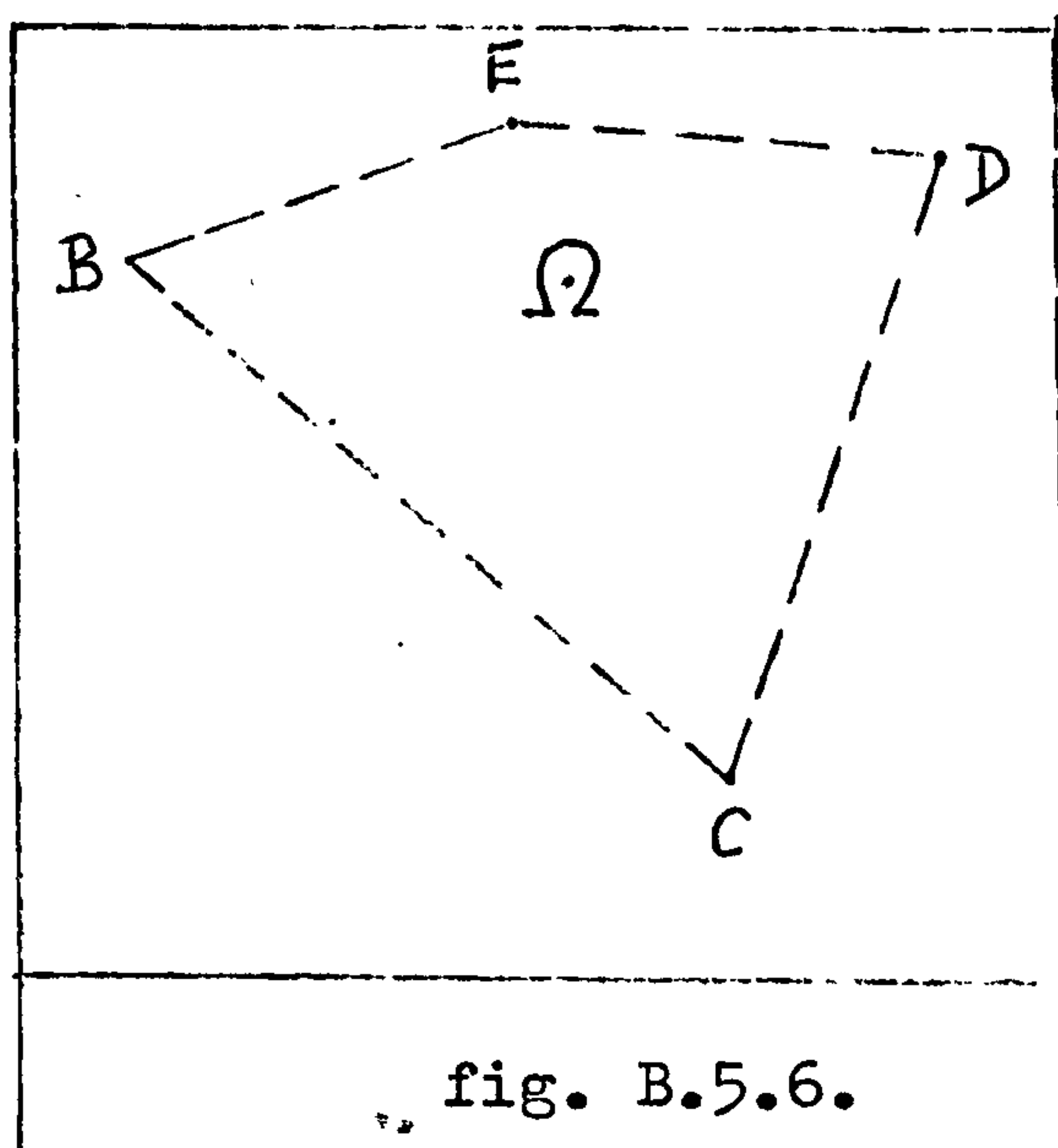
1. This was the method I adopted in my study of the Peshitta Psalter. I now feel, however, that there is something to be said in favour of admitting to the second domain only the variants of the second category. In that case the two domains will not overlap at all, and the two locations will be completely independent of each other. In any case, the second domain is likely to be far greater than the first, and the figures derived from the larger domain are unlikely to have been affected at all decisively by the contribution due to the smaller.

Precautions against the danger of circularity can also be taken when one chooses the readings of Ω in these two domains (particularly the second). The investigator must not allow himself to be influenced in any way by the question of which, and how many, mss support each of the rival readings. It need hardly be stressed that a predilection for any one ms could be disastrous: the probable result would be that Ω would appear in the immediate vicinity of that ms, and this in turn would naturally lead to the acceptance of the reading of that favourite ms as original in a great many passages. Again, it is dangerous at this stage to be unduly impressed by the fact that a certain reading is attested by the majority of the mss; this could result in Ω being located too close to the centre of the map - or so I imagine. In short, we must seek at this stage to make a provisional choice on purely intrinsic grounds in as many passages as possible, with the mental reservation that we may later have to revise our opinion.¹

Let us suppose now that Ω has been satisfactorily located. We shall now consider in detail the problem of estimating its reading from the map. To take an example:

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1. In my study of the Peshitta Psalter, I found some instances in which the map suggested that my earlier choice of original reading had been at fault, e.g. variants nos II and XVIII in the list on pp. 7:21 ff. This is comforting, in that it shews that the map technique is not a mere rubber stamp which will always confirm our initial judgments, whether they are sound or not.

Let us imagine that A is lost but that its map position has been more or less correctly estimated. It will be seen that Ω (which is here synonymous with A) is surrounded by extant mss. We now seek a small set of mss, in the vicinity of Ω , such that the polygon formed by joining up the points on the map will include Ω "comfortably". The smallest set to do so consists of the four mss BCDE, as will be seen from ¹ fig. B.5.6.

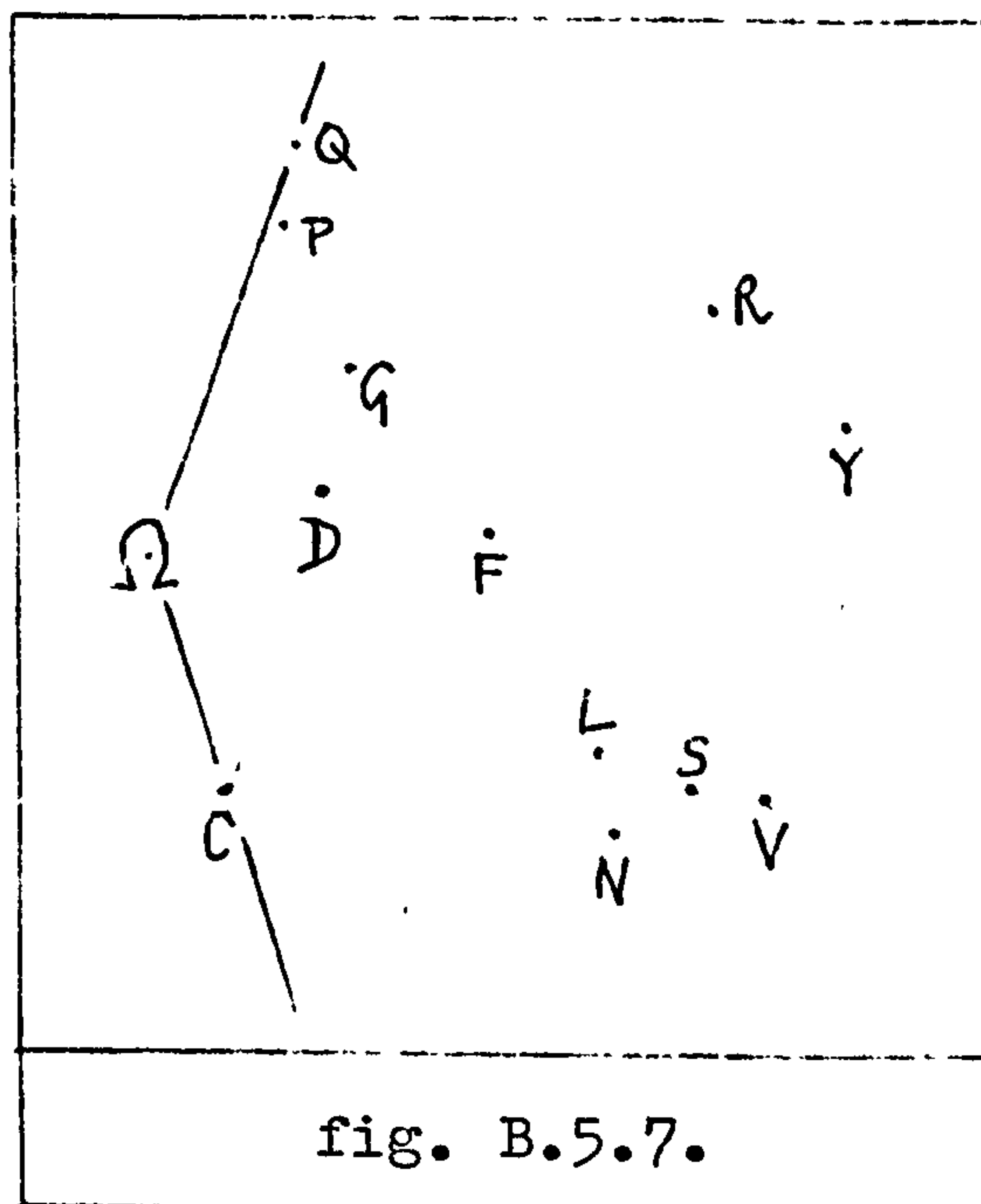


We shall refer to this set of mss as the team.

It sometimes happens that Ω is not surrounded by extant mss, but lies away from the mss of the map. This was the case in the map of the Peshitta Psalter.

1. It may be objected that E is superfluous, in that the triangle BCD itself includes Ω . But Ω would then be close to the side BD, and thus not "comfortably" - again an intuitive term - inside the polygon. This would be unsatisfactory, as the succeeding paragraphs show.

In that event we must choose for our team a small number of mss which, when joined up, span the greatest possible angle around Ω . Thus suppose that of all the mss, seven are lost, viz ABEHKOX. A map of the remaining mss would resemble¹ fig. B.5.7, which has been extracted from the original map and denotes by Ω the position of A. We might then choose as a team the mss CQ:



To these, D may perhaps be added, in that the line CQ will run pretty close to Ω , and a path CDQ would span the angle about Ω rather more "comfortably".

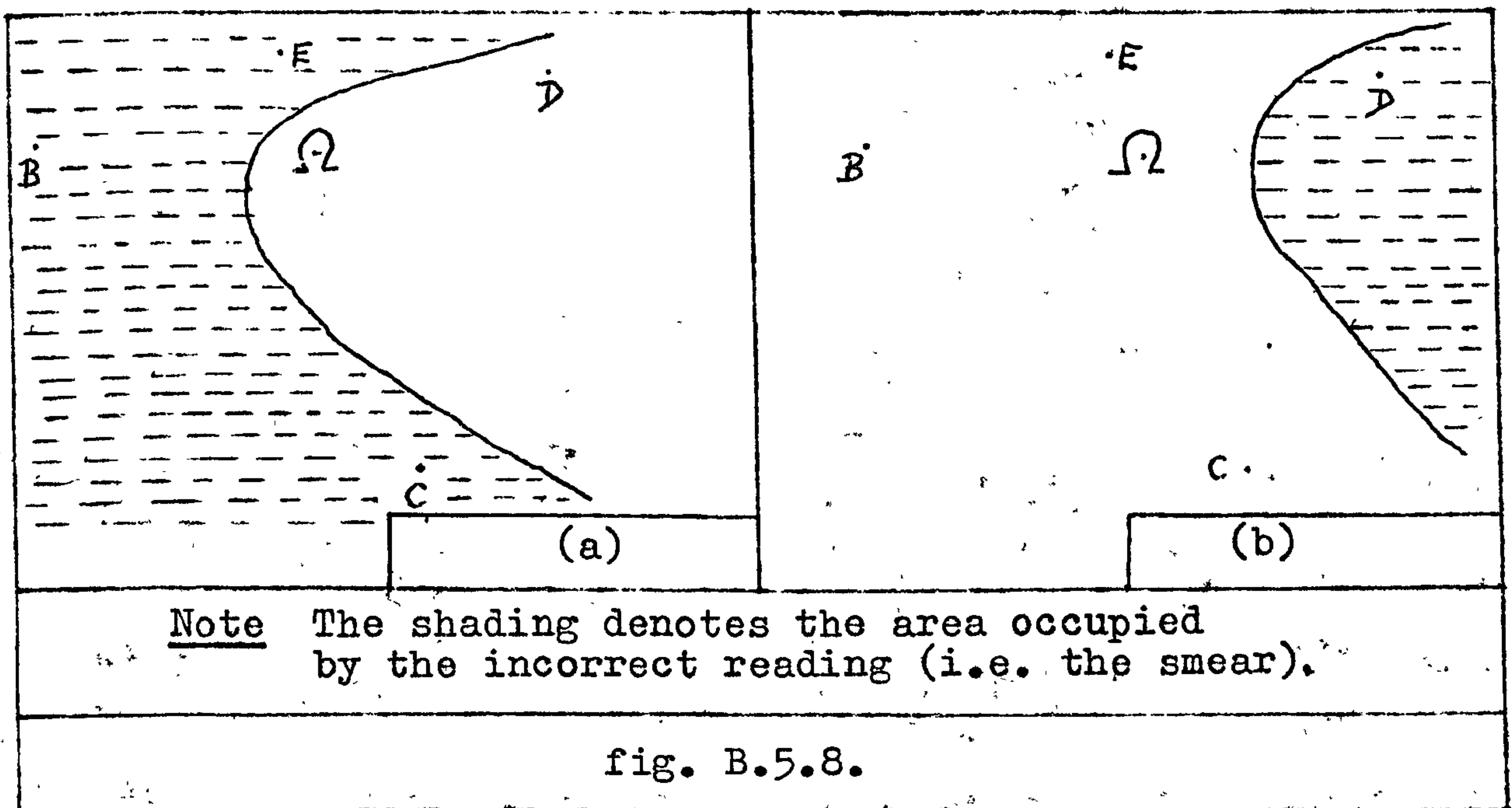
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1. This involves a proposition which is hardly capable of proof but seems sound intuitively, viz that if we have a map of n mss and then omit from it m of the mss (where n is, say, 15 or more, and m is small in comparison with n), the resulting map of the $(n-m)$ mss will be substantially the same as that to be obtained by constructing a new map of those $(n-m)$ mss without reference to the others. By locating additional witnesses (fragments etc.) we do in fact make such an assumption, for $m = 1$. (The number n will then include the fragment which we are trying to add to the map.)

If, in any variant passage, all the mss of the team are in agreement, then the region occupied by their common reading will include the polygon and hence it will include Ω .¹ Thus the reading shared by these mss is likely to be original. We must not, however, overlook two possibilities: (a) the mss of the team agree by coincidence in error, so that what appears to be a "simple" region is in fact made up of several smears; (b) all the mss extant have an error because they all depend, totally or partially, on a common source (p. A:66). Both these considerations must be borne in mind equally in "stemmatic" traditions.² Thus it is unlikely that the original reading survives among the extant mss without being represented at all by the team. It is, I repeat, unlikely; but it is not impossible. We have stressed again and again that such a deduction from the map cannot be regarded as a "règle de fer". All that can be said is that the evidence of distribution is in favour of a reading common to the team; and this is to be borne in mind by the editor, together with whatever other evidence there may be.

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1. sc. in the case wherein Ω is surrounded by extant mss. Otherwise (cf fig. B.5.7.) the reading common to the team occupies an area which stands between Ω and those mss which have another reading; hence the team reading is the most likely to have been that of Ω .
 2. Indeed the second possibility is less serious here than in the case of stemmatic traditions. Thus G. Zuntz observes, in *Classica et Mediaevalia* iii (1940), p. 24: "If on this account [sc. of contamination] a pure genealogy of witnesses is unattainable, these lively exertions, on the other hand, almost exclude the possibility that any old reading, true or false, could have entirely disappeared."

Applying this reasoning to the apparatus for the model text, we shall deduce correctly the reading of Ω in all the thirty-odd passages wherein BCDE agree, such as sublati^s (against sublatas) in l. 10. This model tradition, admittedly, is of course far more straightforward than the textual traditions which one encounters in real life.

If the mss of the team disagree among themselves, then we cannot determine at all confidently which, of all the regions into which the map divides itself at that point, would have contained Ω . Thus, if BCE agreed against D, we would have to recognise both the possibilities shown in fig. B.5.8. However, it seems reasonable - if there are no



cogent intrinsic grounds to the contrary - to prefer a reading supported by the majority of the mss of the team. If we follow this suggestion, we shall correctly identify the original reading in all the remaining twenty-odd variant passages of the model tradition. Nevertheless, less likelihood attaches to a majority verdict than to a

unanimous one; and any reading attested among the team deserves careful consideration.

The reader may well feel that the argumentation leading to this concept of a team of mss is so complicated that one is left unsure of the rationale behind it. There is, however, another way of viewing this procedure, in the light of the work of Bévenot - who has himself advocated the idea of a team of mss, but on very different principles.¹

Let us consider the question of the historical connection between a pair of mss. This may be considerable, e.g. if both are copies of the same late ms, and thus have a lengthy common ancestry; or the connection may be slight, e.g. if the two mss belong to two quite different branches of the tradition, or (more specifically) if their respective source-complexes have but little in common. It is only rarely that we shall find two mss whose lines of transmission from the original are completely independent, i.e. such that the only ms common to the two source-complexes is the original itself. Nevertheless, some ms pairs will be more mutually independent than others. In his study of Cyprian, Bévenot identified certain ms teams, each consisting of three mss,² such that the transmission line from the original to each member of the

1. See Thes., pp. 2:32 f., and pp. 11:113 ff.

2. He later decided to work with a single team consisting of four mss (eapY); see his paper in *Studia Patristica X* (ed. F. L. Cross), Berlin 1970.

team was, as far as possible, independent of the transmission line to every other member of the team. If we found agreement between all - or even the majority of - the mss of such a team, the likelihood that their common reading was original would indeed be considerable.

This, I believe, is the spirit, if not the letter, of Bévenot's theory; and it seems that the ms teams identified by the method of this Chapter are also valid teams in the sense of Bévenot. They will tend to consist of mss which lie, as far as the data available will allow, (1) close to Ω , and (2) in substantially different directions away from Ω . In view of what we said about the relationship between the ancestry of a ms and its position on the map, condition (2) will ensure that no two members of the team are related at all intimately. Meanwhile condition (1) will mean that the mss chosen tend to be among the least corrupt.¹ Thus we shall obtain a team of mss which combine purity and mutual independence to whatever extent our material allows. In the example considered above - where all the mss of the model tradition survive except for A - we obtained the team BCDE, which consists of four independent direct copies of the original. In practice, of course, the situation will not be so favourable; but the methods advocated here set out to yield the best possible team that can be extracted from the

1. as far as possible; but (1) cannot override (2). Thus the team will not necessarily consist of the three or four "best manuscripts" (whatever one may understand by that term).

mss at our disposal. This allows us to appreciate from another point of view why agreement between the mss of the team may be expected to yield the original reading.

When the principles underlying our use of the map are regarded in this light, it is clear that they are by no means alien to text-critical practice hitherto. They seem to have been present, at least in embryo, as long ago as J.A. Bengel's work on the New Testament¹; the twelfth of his "monita" on choosing between rival readings embodies several ideas, and is in parts reminiscent of - though not in complete agreement with - what has been proposed in this Chapter: "Plures ita demum testes paucioribus; et, quod gravius est, testes climatibus, seculis, linguis, diversi, propinquis inter se: et, quod gravissimum est, antiqui novis praeferendi sunt. nam quum Fontis nomen prima tueatur manus, Graeca utique; ab ea quo propius absunt rivi, id est, codices ex primaevis deducti, Graeci, Latini etc. hoc plus habent ponderis." Again, some of the textual critics I have met tell me that they evolve, with experience, an intuitive idea of the interdependence of the mss they employ, so that they learn eventually to judge in what degree of esteem one should hold the agreement of any particular combination of witnesses.² The importance of the map consists in lending to the procedure a degree of exactitude, and in limiting as far as possible the scope of subjectivity.

1. "Gnomon Novi Testamenti", London 1862 (3rd ed.), p. xiii.

2. Dr. Dawe, in conversation with me, compared the development of this awareness to the process of learning a language.

We now conclude by attempting to answer the question which we set ourselves at the beginning of this Chapter: What progress have we made in respect of the three desiderata considered at the end of Chap. 3? As for the first - a critical policy which utilises the evidence of distribution - the map holds out the prospect of being helpful; but first it will be necessary to determine the position of Ω . The second desideratum is a history of the text. This too requires us to locate Ω , whereupon we may be able to offer an outline history, in the manner suggested towards the beginning of this chapter; but unless we have a certain amount of external evidence in addition,¹ we shall not be able to speak of it as the textual history.

Finally, there is the need to keep to a minimum the labour of collation. Here too the map can be useful. We have seen that it is unlikely that the original reading will be attested among the extant mss without being represented among the mss of the team. Suppose now that we have a lengthy series of writings which were customarily copied on to a single ms - e.g. a collection of plays by a Greek dramatist, or a set of theological tractates by the same Church Father. Let us imagine further that we have established a map, and thence a team, for one of the constituent works. If we now wish to study the text of another work in the series, it is

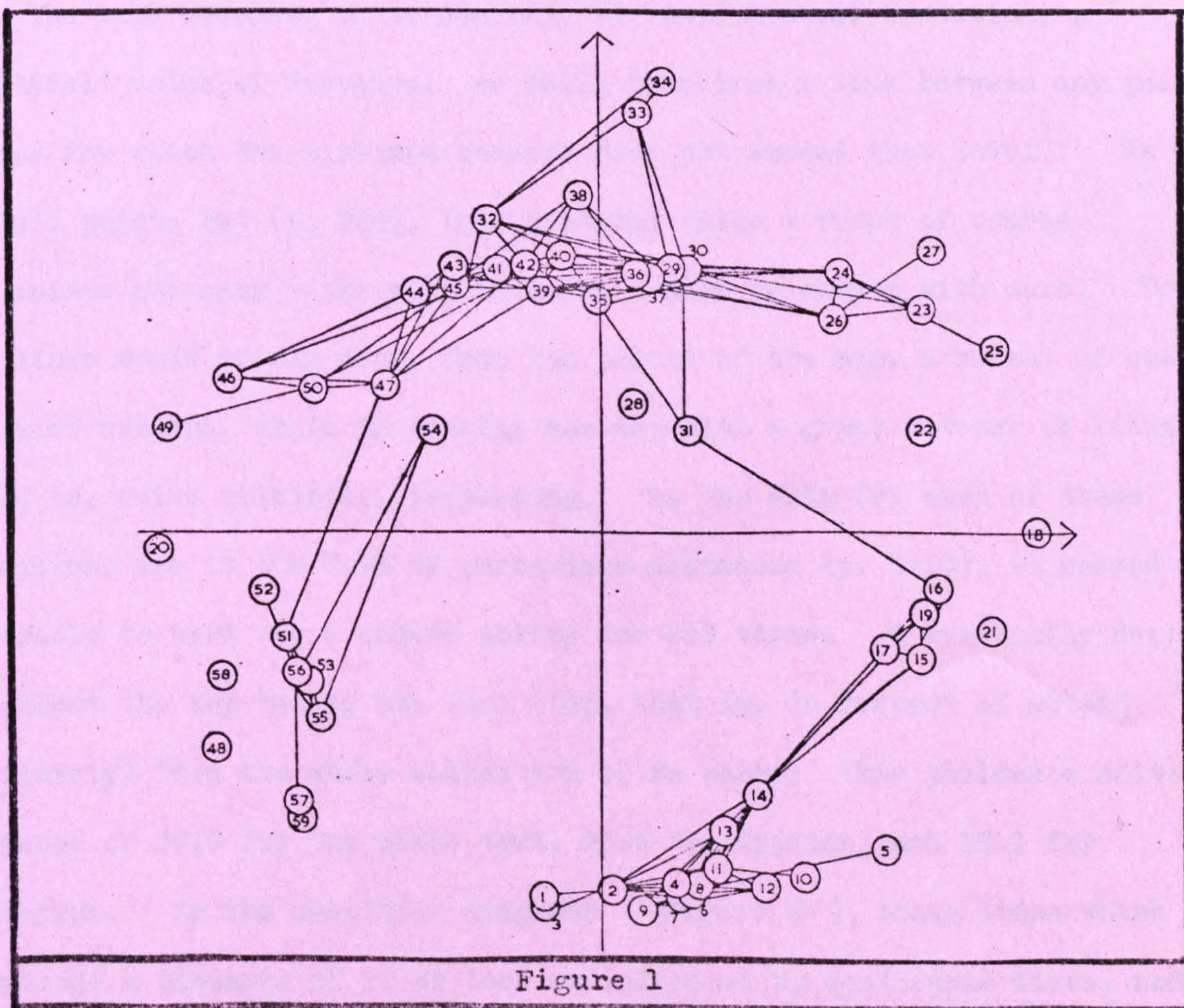
1. such as the date of each ms, its provenance, and (if applicable) its association with a particular sect.

natural that we should start by consulting those mss which found a place in the team for the work already examined. This does not mean that we are thereby authorised to dispense immediately with any ms excluded from the team. That proposition might have been warranted if we could have been sure that the inter-manuscript relationships were constant throughout the series. But, as we have shewn on pp. 3:22 ff, changes of affiliation frequently occurred; whence it follows that if we wanted to be certain of not missing any original reading that has survived among our mss, we would have to collate them all. On the other hand, the price of that certainty is formidable. The exhausting task of collating mss is subject to a law of diminishing returns; a point is reached, sooner or later, when the investigator must seriously question whether the collation of a further ms will repay, in terms of knowledge of the original text, the effort invested. Thus there is some demand for a method to identify those mss which seem the most likely, a priori, to give attestation, between them, to the greatest number of original readings. It is for this reason that one would be well advised to begin with the mss of the team. Moreover, if we subsequently wish to collate more mss, the map may help us in selecting mss which are not intimately related to one another and not unduly corrupt.

But now it is time for us to test these theoretical expectations by applying the ideas of this Chapter to real, and not to artificial, textual traditions.

At the oral examination, Professor D. G. Kendall called attention to a device which he, in his own application of MDS to archaeological data¹, had found exceedingly useful, namely the drawing of straight lines to connect those pairs which showed a particularly high degree of similarity. He required that such line segments be added to MDS maps in the present Thesis.

Before setting forth the results, I should explain briefly what part this procedure played in Kendall's study. A number of individuals (in this case, graves) had to be arranged, on the basis of measures of similarity, in a linear series, which it was hoped could serve as a chronological sequence. Kendall began by performing MDS analysis in two dimensions. He obtained a map (reproduced as Figure 1) in which the individuals could



1. "Seriation from abundance matrices", in "Mathematics in the Archaeological and Historical Sciences" (see Thes., p. 11:84), pp. 215-252.

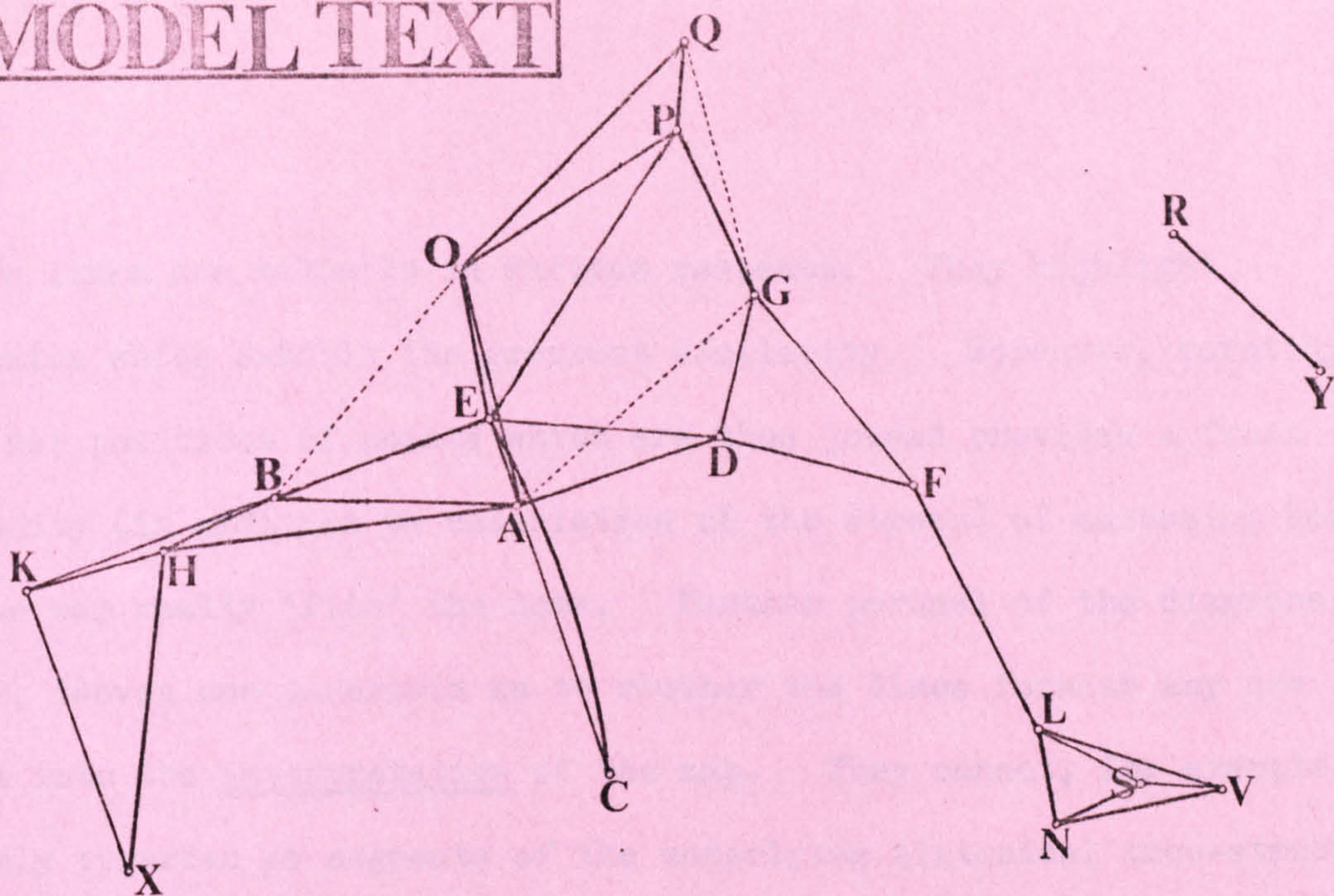
be seen - when the 'strong links' were inserted - to arrange themselves, by and large, in a 'horse-shoe' formation. After his data had been subjected to a certain mathematical transformation (the reader must be referred to the paper itself for details), he obtained from it a new map, in which the 'horse-shoe' had become 'unbent' so satisfactorily that he was able to describe the configuration as "almost linear" and to derive from it the sequence which he had sought. To descry these patterns would have been far more difficult if the 'strong links' had not been drawn in.

I therefore proceeded to examine the effect of thus augmenting my MDS maps relating to the model, Cyprian and Aeschylus texts. The maps appear in their 'plain' form on pp. 5:7, 6:4, and 6:40 respectively.

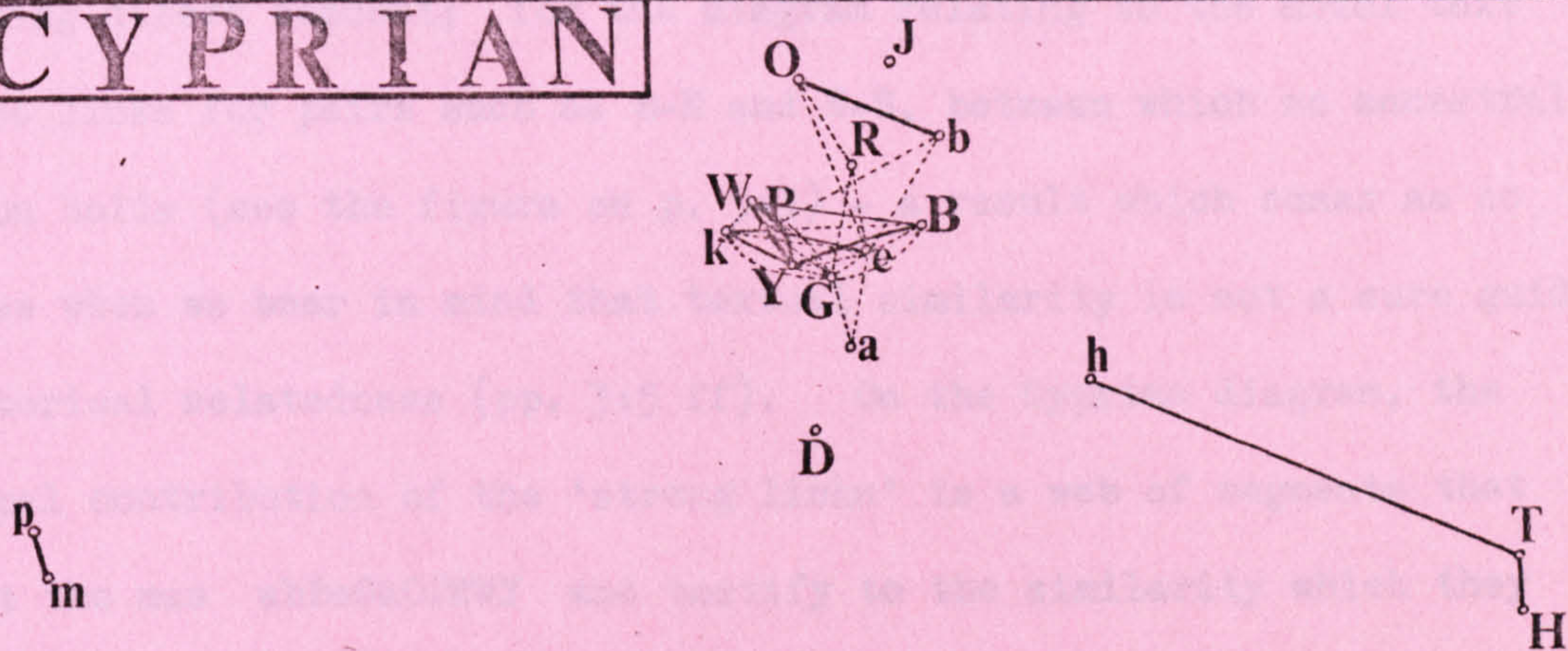
The task requires us to specify, for each textual tradition, a 'critical' value of distance; we shall then draw a link between any pair of mss for which the distance measure does not exceed that level¹. As Kendall points out (p. 228), this critical value - which of course determines how many links will be drawn - must be chosen with care. Too few links would merely form, from the points of the map, a number of small isolated bunches, while to overlay the map with a great network of links would be, quite literally, perplexing. As the data for each of these traditions was in the form of percentage distances (p. 3:10), it seemed desirable to work out a common policy for all three. I eventually decided to select the top twenty per cent (top, that is, in respect of mutual similarity) from the whole collection of ms pairs; this yielded a critical distance of 22.0 for the model text, 23.6 for Cyprian, and 28.1 for Aeschylus. On the resulting diagrams (Figure 2), those links which represent a distance of 20 or less are indicated by continuous lines, and the others by thinner broken ones.

1. The distance measures for these particular pairs will, of course, have already been taken into consideration - together with all the other distance measures - when the map is compiled by the computer; but our task is now to indicate them explicitly.

MODEL TEXT



CYPRIAN



AESCHYLUS

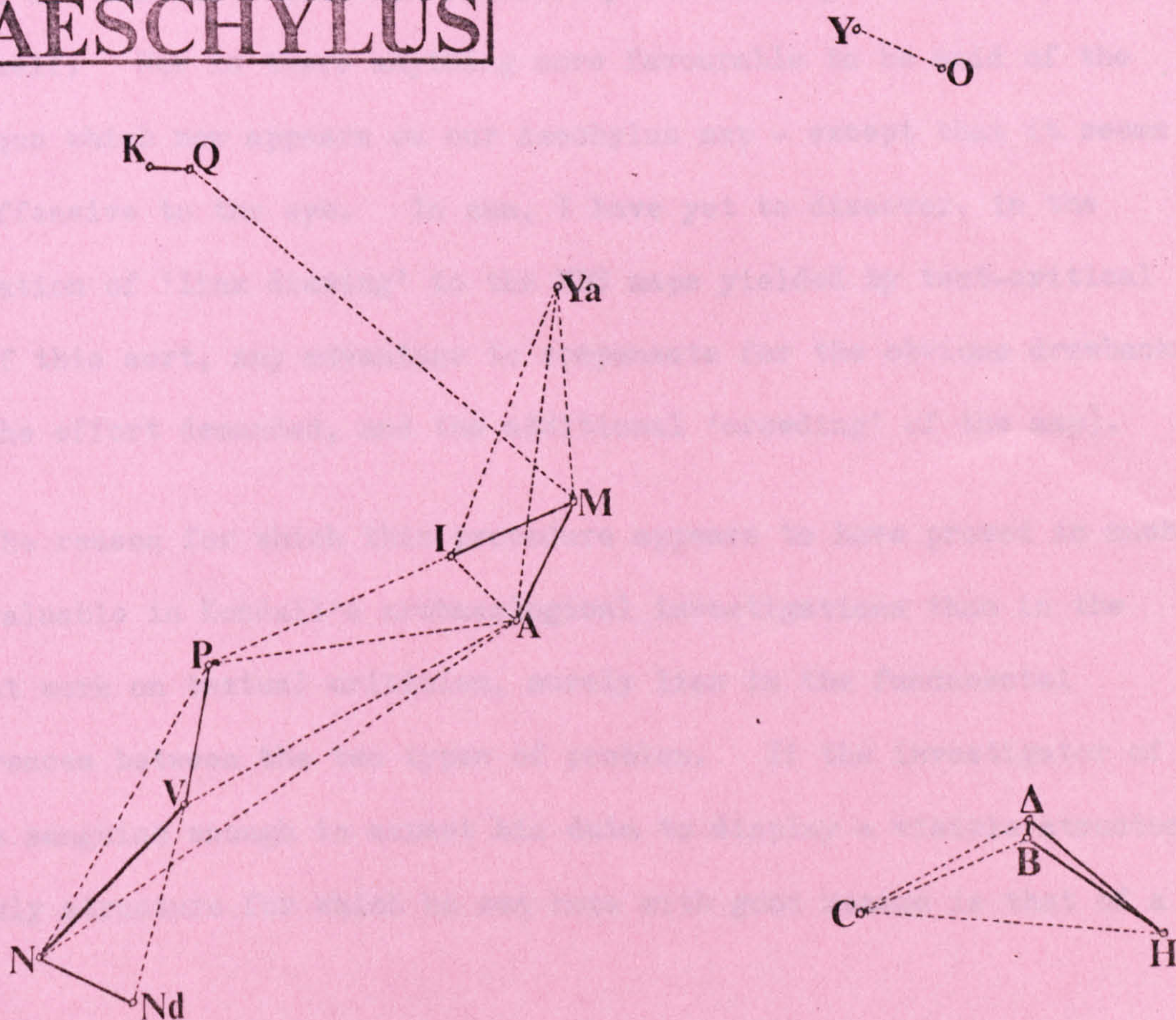


Figure 2

The links are valuable in various respects. They highlight those pairs which exhibit the greatest similarity. Moreover, scrutiny of the map positions of points which are thus joined provides a fresh opportunity (in addition to calculation of the stress) of assessing how well the map really 'fits' the data. Further perusal of the diagrams, however, leaves one uncertain as to whether the lines furnish any new insight into the interpretation of the map. They cannot, for example, be safely regarded as segments of the underlying historical tree-structure, indicating direct descent; for the diagram relating to the model text includes links for pairs such as B-E and N-S, between which no ancestral relation holds (see the figure on p. 5:3) - a result which comes as no surprise when we bear in mind that textual similarity is not a sure guide to historical relatedness (pp. 3:5 ff). On the Cyprian diagram, the principal contribution of the 'strong links' is a web of segments that connect the mss abBeGkOPRWY and testify to the similarity which they bear to one another - but that similarity had already been conveyed by the map itself. Nor is there anything more favourable to be said of the arabesque which now appears on our Aeschylus map - except that it seems less offensive to the eye. In sum, I have yet to discover, in the application of 'link drawing' to the MDS maps yielded by text-critical data of this sort, any advantage to compensate for the obvious drawbacks (viz the effort demanded, and the additional 'crowding' of the map).

The reason for which this procedure appears to have proved so much more valuable in Kendall's archaeological investigations than in the present work on textual criticism, surely lies in the fundamental differences between the two types of problem. If the investigator of mss is sanguine enough to expect his data to display a visible structure, the only structure for which he can hope with good reason is that of a

tree, not a line¹; and should contamination render the stemma an inadequate model, that fact in itself will not provide any ground for supposing that the points on the map will now exhibit linearity (or any 'bent' variation thereon). Thus the service which the links perform in Kendall's work, where they enable us to see the 'horse-shoe' take shape and evolve, is not applicable here; no alternative function of comparable value can be assigned to them; and so it seems best, on the whole, to present the MDS map which arises in textual criticism as a configuration of points alone, without the imposition of line segments.

1. notwithstanding the seriations carried out by J. G. Griffith (pp. 11:123 ff.).

6. PRELIMINARY STUDIES

A few points deserve to be made clear, by way of introduction, before the reader proceeds to the studies on these four texts. Firstly, it is our method which is under scrutiny here, rather than the textual traditions themselves. Our primary objective is to gain some insight into its workings, and to assess its potentialities and limitations; any contribution which may be made to our knowledge of the actual texts is to be regarded as a mere by-product. Secondly, the reader is reminded that in the first two studies I started out from an apparatus criticus, from which I compiled my own figures, while for the last two I relied on the numerical tables of other workers. For that reason, rather more is said here on the Cyprian and Aeschylus texts than on the Vulgate Isaiah and the Gospel of Luke; experience seems to show that statistics compiled by another, no matter how conscientiously, cannot be so readily utilised and interpreted as data which one has worked through in person. Lastly, I claim no expert knowledge of any of these traditions - though I would not have ventured to write on any of them if I had felt completely at sea - and so I have simply done whatever I have been able to. This has, I fear, resulted in a certain unevenness of treatment, which may well infuriate the specialist; but it seemed to be a price well worth paying in return for the variety of the material.

A. CYPRIAN: DE UNITATE

References

- M. Bévenot, "St Cyprian's De Unitate: Chap. 4 in the Light of the Manuscripts" [= The Bellarmine Series, no. 4], London 1937. (cited as SCDU)
- ibid., "The Tradition of Manuscripts: A Study in the Transmission of St Cyprian's Treatises", Oxford 1961. (cited as TM)

We have already referred more than once to the textual tradition of Cyprian's treatise "De ecclesiae catholicae unitate"¹, which has been investigated in great detail by Prof. M. Bévenot. As Bévenot points out (TM, pp. 6 f), the mss refuse to be arranged in the form of a stemma, so that this tradition will provide an excellent test for our novel technique of textual analysis. Bévenot himself, seeking to cast light on the problems of choice between rival readings, propounded his theory of 'disconnexion' or 'opposition' of manuscripts; that part of his contribution is discussed below (pp. 11:113 ff). In this Chapter, however, we shall confine ourselves to the analysis of the tradition by MDS.

The present study was based on the eighteen mss which Bévenot selected, out of some 160 that are extant, to be reported fully over the whole treatise. These are listed in Table B.6.1, together with their probable dates and the families into which Bévenot divided them according to a scheme worked out in SCDU:

1. The treatise was written 251 A.D.

TABLE B.6.1.

Manuscript	a	b	B	D	e	G	h	H	J	k	m	o	p	P
Century	12	12	11	9	12	9	11	12	9	11	11	9	11	9
Family	2Xc	5Xa	6Xa	2Ca	5B	2Bb	7A	1A	5Xf	5(C)a	2Bc	5Xb	2Bc	2(C)a

Manuscript	R	T	W	Y
Century	9	9	9	9
Family	2Xa	4A	2Ba	2Bs

In addition, Cod. S¹ (cent. v-vi) is extant almost to the end of Ch. 5 of the work; there are also a few collations of the lost Verona ms (V), apparently of the sixth century².

I recorded variants from the whole treatise, except for Ch. 4; that Chapter has been handed down in two quite different versions, and the mss diverge too widely for one to be able to count up agreements and disagreements in the customary fashion. The policy followed in admitting variants to consideration has already been described (pp. 2:41 f). The eighteen mss yielded altogether 444 variants. From these, percentage distances were computed³, and thence the two-dimensional map⁴ shown in fig. B.6.1.

1. E. A. Lowe, *Codices Latini Antiquiores*, vol. v, no. 602.

2. See TM, p. 50, n.7.

3. Absolute distances would have done almost as well, since all eighteen mss are extant over virtually all the variant passages.

4. For a three-dimensional map of this tradition, see p.4:18.

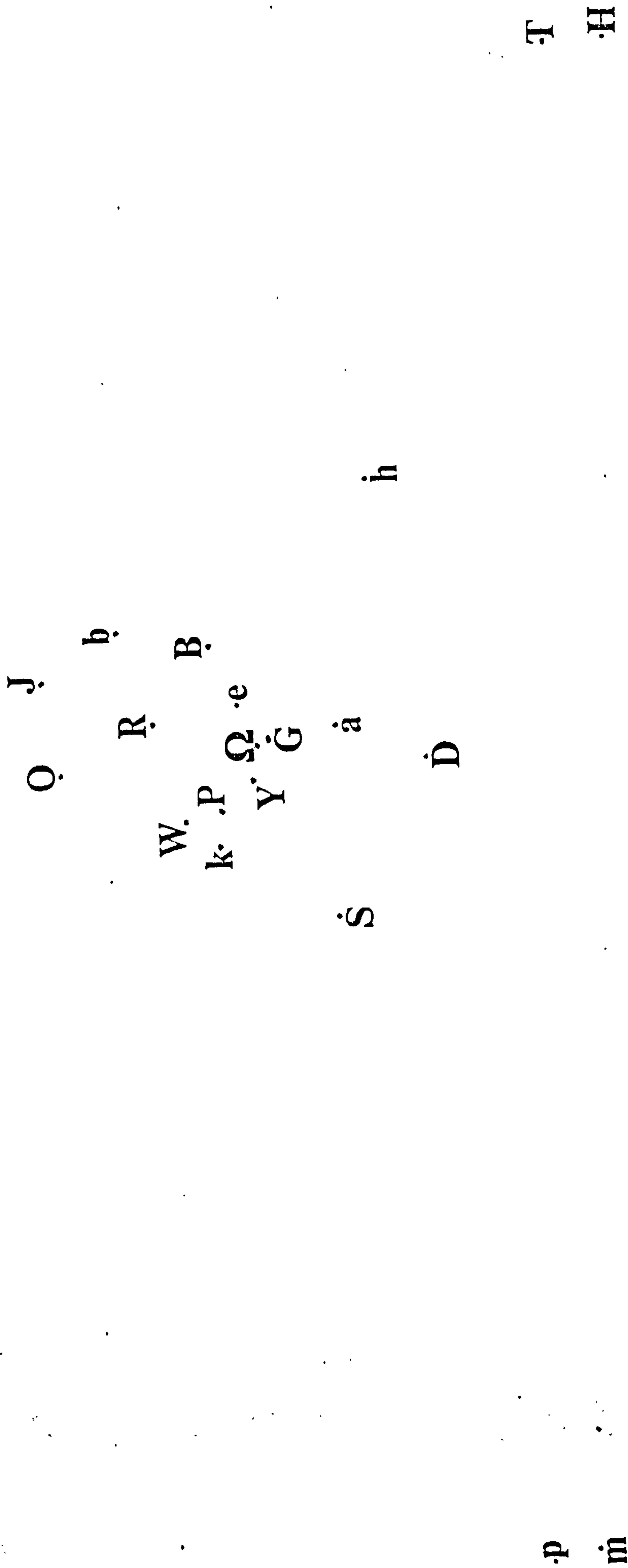


Fig. B.6.1

We may seek in addition to locate the ancient Paris Codex, S, which is extant over the first 76 of our variant passages. Its disagreements¹ with the eighteen mss were found to be:

a 19 b 23 B 20 D 18 e 17 G 17 h 25 H 28 J 13
k 20 m 26 O 25 p 25 P 20 R 14 T 30 W 16 Y 14

The position of S shown on the map was reached by means of the second 'fragment' technique (p. 4:9). It did not however prove possible to locate V, whose readings are given at no more than 7 of the 444 points of the text.

The next task is to locate Ω . It will be recalled that this normally involves our deciding provisionally, in a number of passages, which of the rival readings is the oldest. Ideally, the collection of passages concerned should form no more than a small fraction of the total, and should consist of cases wherein we can be well-nigh confident of our decision. Perceiving, however, that it lay beyond my own competence to make the necessary judgments, I fell back on the "Resultant Text" which Bévenot compiled, i.e. a text which gives, at every point at which the mss diverge, the reading which he regards as the best of those offered by the mss. Thus the field of variants in which the reading of Ω was provisionally selected, came to consist of all 444 passages. As we observed above (pp. 5:14 ff) this involves a serious danger of circularity in some of the

1. I fear that these figures, and others presented at subsequent points in this chapter, may be slightly, but hardly seriously, inaccurate; experience shows that in recording the behaviour of mss and making counts of this sort, errors can all too easily arise from time to time.

later stages of our argument; the danger will have to be borne in mind, but, as we shall see, it could hardly be said to vitiate our conclusions altogether.

On the basis of the whole treatise, then, the number of times that each ms differed from Bévenot's Resultant Text was calculated:

a 70 b 91 B 70 D 85 e 63 G 62 h 81 H 131 J 83
k 78 m 126 O 87 p 124 P 67 R 70 T 125 W 69 Y 67

(S diverges from the Resultant Text on 12 occasions, over the 76 variants for which it is extant; scaling this up for 444 passages, we would obtain a figure of 67.)

The place which was assigned to Ω through the second 'fragment' technique is shown in fig. B.6.1. It is remarkable how close Ω comes out to G.

Let us now look at our map in the light of two criteria which have been appealed to in the classification of the mss, namely (i) the order of the treatises, and (ii) the text of Ch.4.

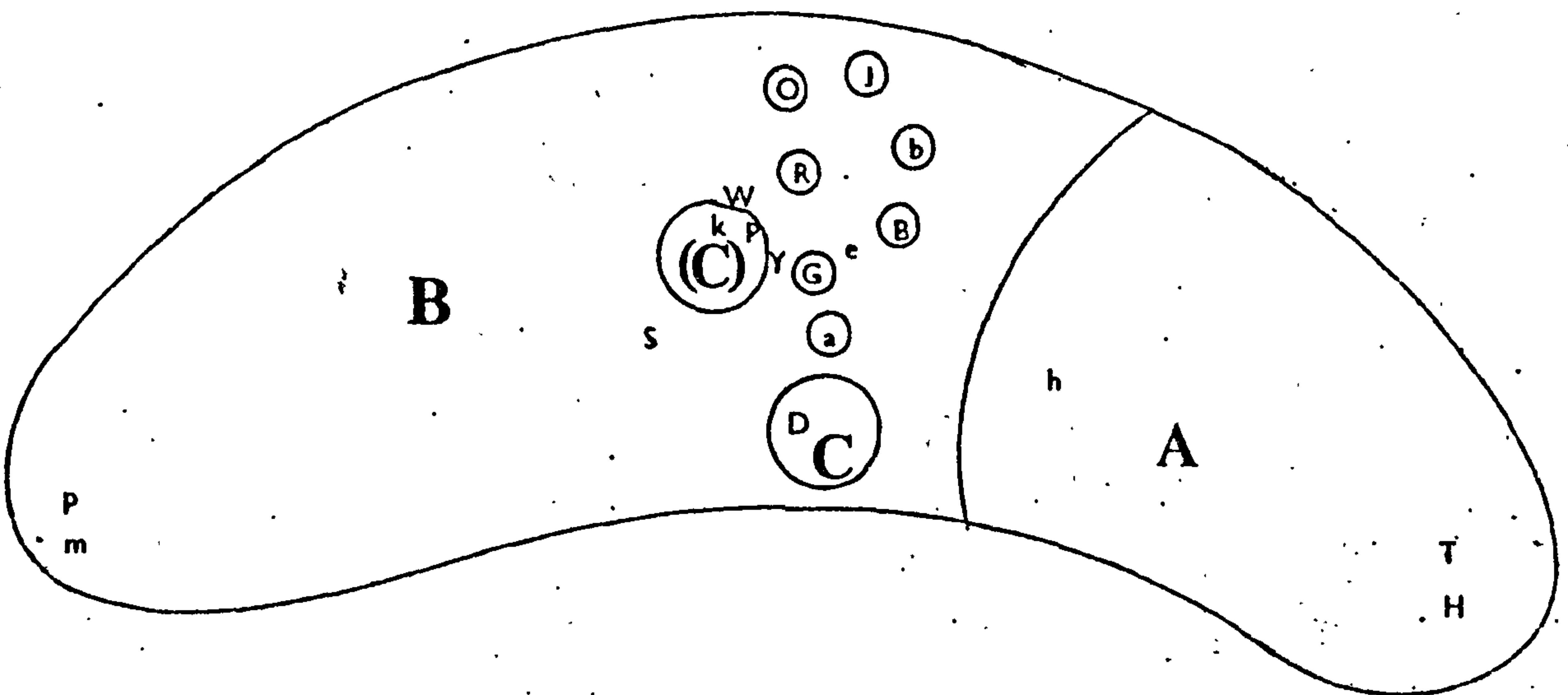
The order in which the treatises of Cyprian appear differs among the mss. We may therefore group together mss which present the treatises in the same order. On that basis, hHT go together in that they all arrange the treatises in a particular order which Bévenot denotes A¹; another order, denoted B, is found in empSYW. D and the 'Cistercian' mss

1. A symbol denoting the order in which the treatises appear in a given ms constitutes the second element of the code for the family to which that ms is assigned (Table B.6.1).

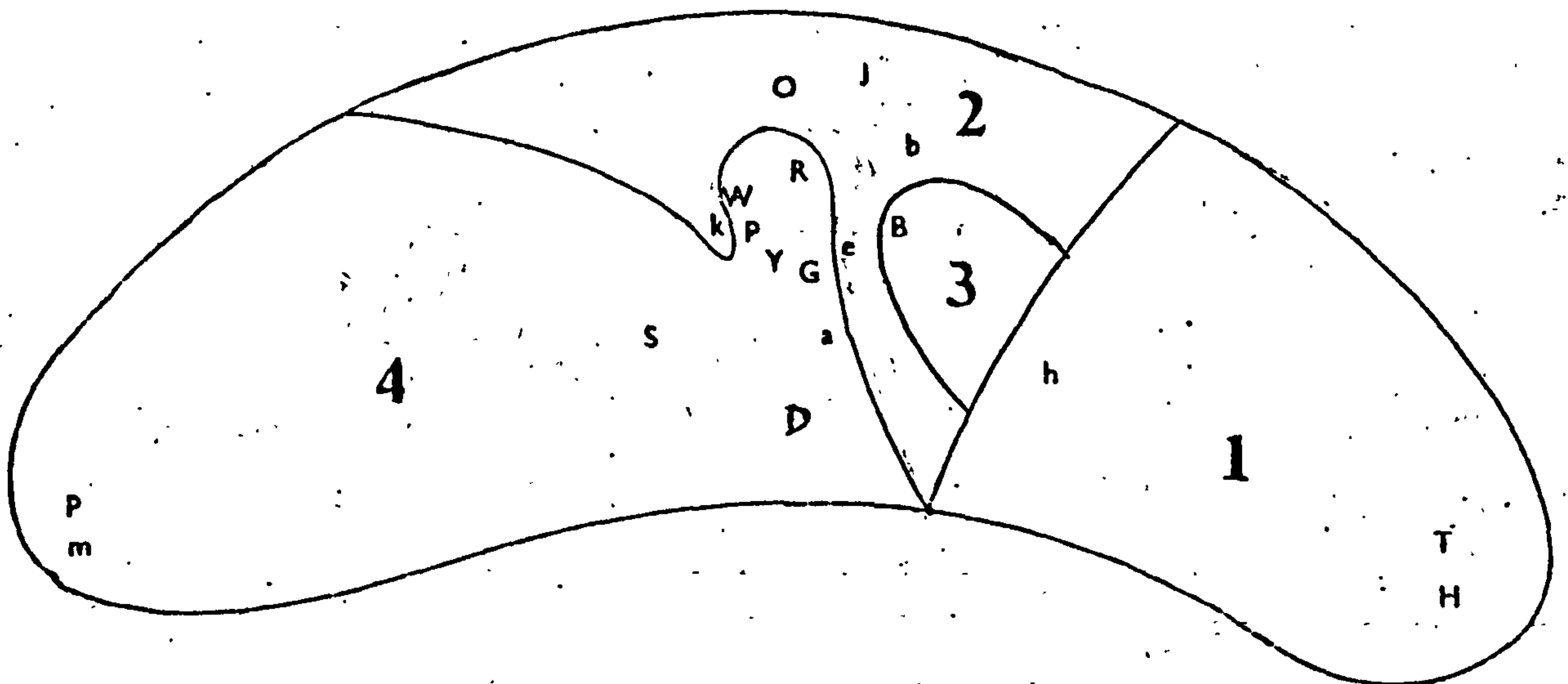
give order C, while the order common to the mss kP is denoted (C), on the grounds that it could have resulted from C by an easy displacement¹. Each of the remaining mss abBG²JOR offers a different order, peculiar to itself³. Fig. B.6.2a, in which the map has been divided into regions on the basis of this classification, allows us to make some tentative remarks. The area covered by mss of class B is rather large, but interrupted by representatives of classes C, (C) etc.; thus the mss of class B do not all resemble one another particularly in text. Furthermore, the area occupied by (C) does not lie especially close to that of C; one may therefore wonder whether it is indeed appropriate to regard (C) as a variation on C, as Bévenot does, or whether we should rather consider order (C) to be independent of C and denote it accordingly by a symbol of its own (such as E).

Two versions exist of Ch. 4, and Bévenot has shewn that both go back to Cyprian himself, one representing a first edition (denoted PT) and the other a revised edition (TR⁴). There is one ms which gives PT alone; many others have TR, without any trace of PT; the remainder show various combinations of both versions⁵. In fig. B.6.2b, the mss are

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1. SCDU, p.viii, where the alleged relationship is said to be "further confirmed by textual resemblance".
 2. One might therefore have expected cod. G to be assigned symbol X rather than B; the reason for Bévenot's use of the latter symbol is that G shows particular textual resemblance to certain mss which do belong to class B (see SCDU, p. viii).
 3. See TM, pp. 48 ff.
 4. These symbols stand for 'Primacy Text' (in that this version contains the word "primatus") and 'Textus Receptus' respectively.
 5. The number which forms the first element of Bévenot's family symbol for a given ms refers to the form in which it presents the text of Ch.4. The code is explained in TM, p.8.

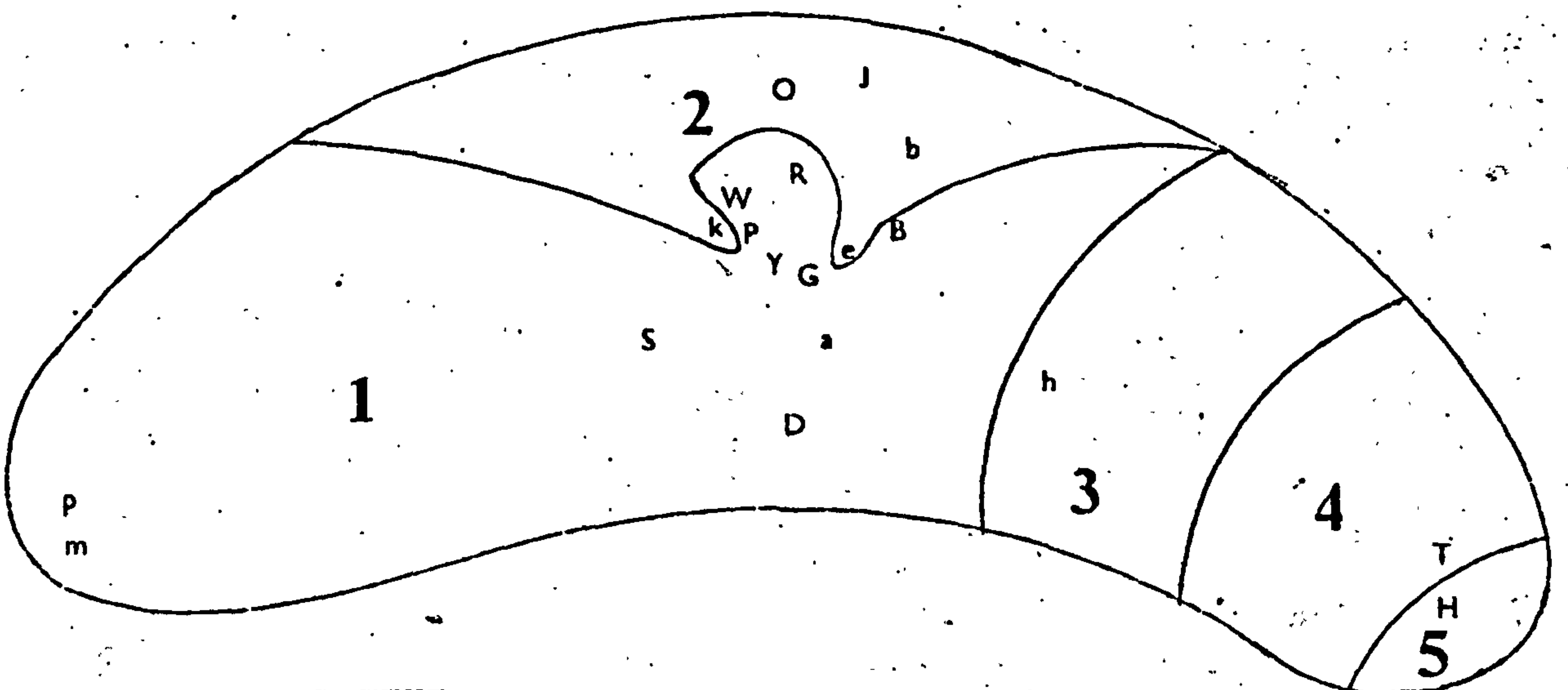


ORDER OF TREATISES. The regions occupied by the classes A, B, C, (C), are shown. A ms which is surrounded by a small circle presents the treatises in an order different from that of all the other mss.



EXTENT OF PT TEXT.

1: entire. 2: the first two sentences. 3: the first sentence. 4: none.



EXTENT OF TR TEXT.

1: entire. 2: all but the first sentence. 3: sections (interlaced within the PT text). 4: all except for one sentence. 5: none.

Fig. B.6.2

classified according to the varying amounts of PT which they contain; fig. B.6.2c similarly shows the extent to which they attest TR¹. It is interesting to observe the various ways in which the map is divided hereby into continuous regions. As one runs one's eye over the map from left to right, the attestation of PT tends to increase, at the expense of TR. Bévenot's conclusion that we can discern both a first and a revised edition of Cyprian's work in Ch. 4 - and indeed, albeit to a far smaller extent, throughout the treatise² - will have to be taken into account when we come to interpret the map.

We may further compare the small groups which Bévenot was able to identify on the basis of obvious textual resemblance, namely m-p, W-Y, k-P, h-H-T, and (within this last group) H-T. With the exception of W-Y³, one can state of each of these groups that its constituent members lie in a particular area of the map and form a cluster that can easily be recognised.

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1. For a full statement of the varying amounts of PT and TR contained in the several mss, the reader is referred to the "fold-out" tables at the end of SCDU.
 2. particularly in Ch. 19; Bévenot offers a tentative list of readings that stood in the first edition, in TM, pp. 140 f.
 3. W and Y are "both derived from the same parent MS" (TM, p. 22); but though they lie fairly close together on the map, each of them appears to be closer to P than to the other. (The same is true of the three-dimensional map on p. 4:18.) This situation is somewhat disturbing, especially since a special relationship between W and Y is picked up by other methods (pp. 2:45, 11:132, 11:144). We may note, however, that the mss divide themselves in the pattern $\Sigma:WY$ less frequently than in any of the patterns $\Sigma:mp$, $\Sigma:kP$ etc. (see p. 2:42), and that the map distance between W and Y is, after all, quite small. (After Greg's notation, the symbol ' $\Sigma:$ ', followed by a list of mss, denotes the agreement of those mss against the rest.)

We now come to the central question: What policy for discriminating between rival readings does the map suggest? The first task is to pick a team of mss which surround Ω "comfortably". Inspection of fig. B.6.1 shows that three members of the team will have to be e, G and Y. It is desirable to add a fourth, for although Ω is contained in the triangle eGY, it is very close to the edge eY. There are two possibilities for the fourth member of the team: P and R. Of these, R seems preferable, in that the bearing from Ω of P is not very different from that of Y, which is already in the team. We thus arrive at a team of four mss: eGRY. It is noteworthy that these four mss each originated in a different country: England, Switzerland, France and Germany respectively. We ought also to keep an eye on hHT, which seem especially likely to embody material from the first edition of the treatise.

Over the 444 variant passages, the reading selected for Bévenot's Resultant Text can be characterised as follows:

(a) it is supported by the whole team (or by as many of its members as are available), 255 times;

(b) it is not the only reading attested within the team but is supported by more team members than is any rival reading, 142 times;

(c) it is found in two members of the team, while a rival reading appears in the other two, 29 times¹;

1. The mss divide themselves eG:RY eight times, eR:GY ten times and eY:GR eleven times.

(d) it is supported by only one member of the team,
11 times;

(e) it has no support from the team at all,
2 times.

The sum of 397 passages (89%) in which the reading preferred by Bévenot has the support of at least the majority of the team, is reassuring, although we do not know to what extent this "confirmation" is due to the fact that our choice of team depended partially on Bévenot's selection of readings in the first place.

The eleven cases in which his preferred reading is found in just one member of the team are¹:

DE ECCLESIAE CATHOLICAE UNITATE ([Y]²: title); in
 Christum (Y: 210.25); spandit (G: 214.13);
 evadet (e: 214.25); totam semel et solidam ([Y]:215.22);
 nuntiat (e: 217.14); serpentes (G: 219.1); constet
 (R: 220.21); sacerdotem (G: 226.13); evacuavit (R: 230.3);
 si caveret, evaderet (Y: 232.26).

As we mentioned above, we must be prepared to find on occasion that a reading with no more than minority support from the team is nevertheless original. Most, if not all, of these eleven readings illustrate that point. It should be noted that each of the four members of the team has contributed to the list, and thereby justified its place in the team; e twice, G three times, R twice, Y four times³.

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1. Bévenot's reading is cited, followed by the one team member in which it occurs and the reference to the page and line of Hartel's edition (CSEL iii).
 2. Y is the only member of the team to have the word CATHOLICAE (which it places before ECCLESIAE). See p. 6:33 below.
 3. Bévenot's own method (see pp. 11:113 ff) led him also to a team consisting of four mss, namely aepY. As far as I can ascertain, the only readings in his Resultant Text which do not appear in at least one member of that team are credulitate (210.4) and evacuavit (230.3).

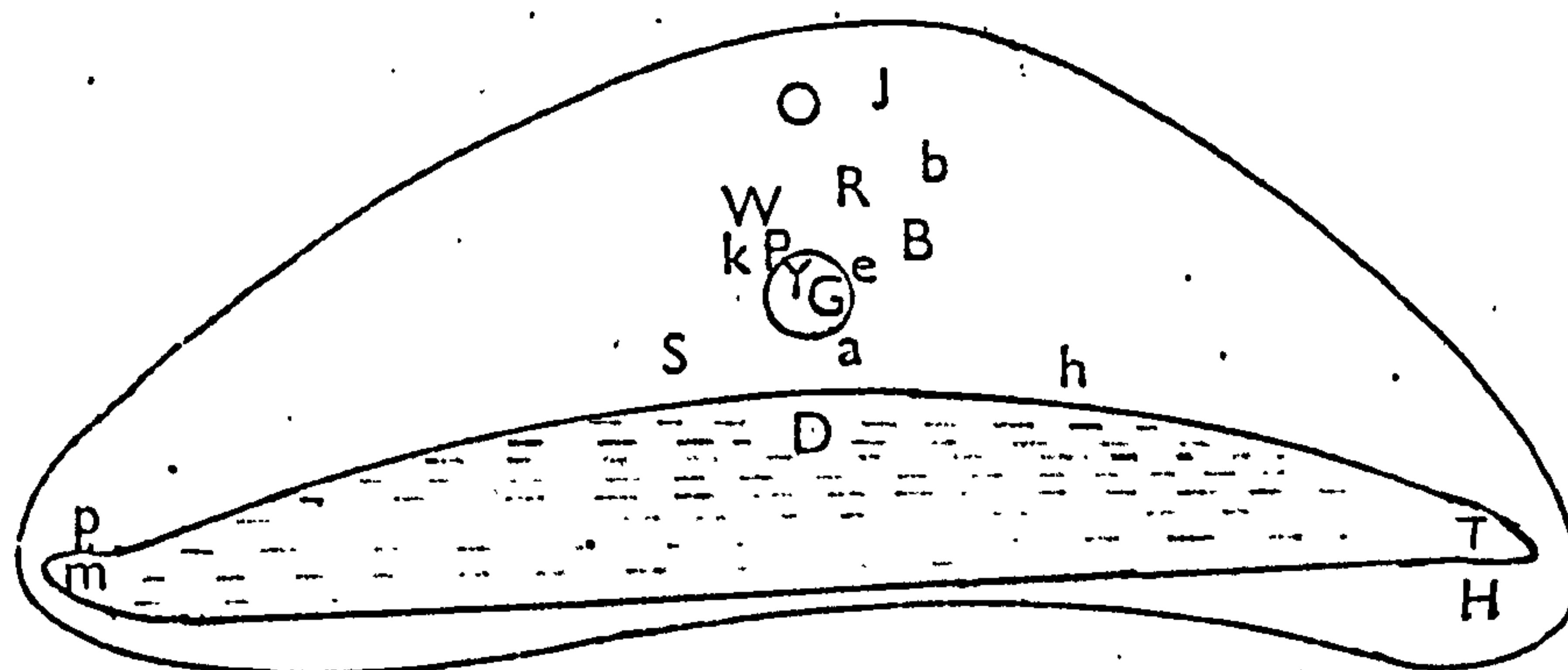
The seven cases in which none of the mss of the team support the reading of the Resultant Text deserve to be looked at more closely. Six are at 210.4, 214.23, 217.4, 218.20 (twice), 233.9; the other concerns the title. All are commented on below; we also take the opportunity of discussing two other passages.

210.4 The Serpent in the Garden of Eden "verbis mendacibus blandiens rudes animas incauta credulitate decepit". This text, which Bévenot adopts, may be translated literally: "flattering with lying words, he beguiled their inexperienced souls by means of their unguarded credulity". Although the construction is a little harsh¹, in that one might have expected that an ablative placed immediately before /decepit/² should refer to some quality displayed by the Serpent ("he beguiled them with..."), the text presents no serious difficulty and may be regarded as original.

Now the mss behave strangely here in three respects. First, rather more than half of the mss which Bévenot examined - including e and R(m.1) - have not /credulitate/ but /crudelitate/, which, as he shews, is unlikely to be original. Second, there are a number of small groups of mss which exhibit great textual affinity in general but are divided on this particular point³.

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1. See Bévenot's discussion of this passage (TM, pp. 65 f). -
 2. The reader is asked to pardon the practice of enclosing Latin words or phrases in slashes, but he will thereby be saved from being momentarily bemused by such forms as "is", "sum" and "eat".
 3. namely mp; HT; WY; D and its congeners; E and its congeners.

Third, the mss present further alternative readings, which are manifest errors. The two remaining members of our team, G and Y, which ought by definition to be largely independent of each other¹, both have the nonsense word /credelitate/ (which a second hand has corrected, in both cases, to /credulitate/); two other mss, not included among the eighteen, have /incredulitate/ and /cruditate/ respectively. Thus the team lends no real support to /credulitate/, the preferred reading²; and all these facts call for explanation.



KEY horizontal shading: /credulitate/. unshaded: /crudelitate/.
 enclosed in circle: /credelitate/. The reading of the first hand alone is recorded.

Fig. B.6.3.

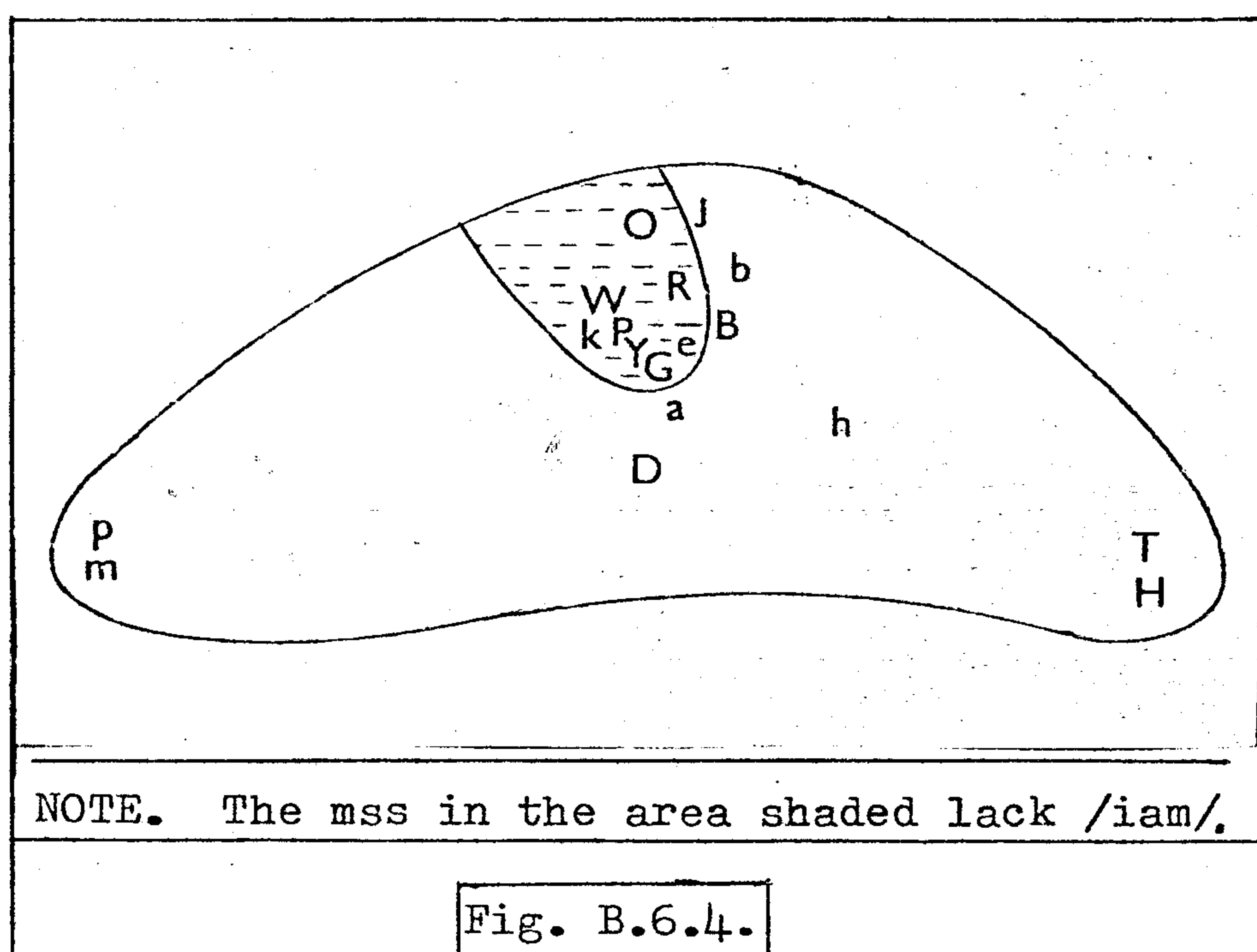
1. There are only 14 places throughout the treatise in which they agree in a reading other than that adopted for the Resultant Text.
2. The same is true of Bévenot's team (aepY).

Perhaps a clue may be discerned in the reading /credclitate/ attested by G and Y. It is possible that in an ancient ms, on which all our eighteen depend, /credulitate decepit/ was miscopied as /credelitate decepit/, the syllable /du/ being conformed, through an involuntary anticipation ("Vorwirkung"), to the initial syllable /dc/ of /decepit/¹. Perhaps the slip was made in the original itself² - though there can be little doubt that Cyprian intended to write /credulitate/. A later copyist or revisor confronted with this reading might allow it to stand; or he might change it, by altering just one letter, to one or other of two real words, /credulitate/ or /crudelitate/. In G(m.1) and Y(m.1), this ancient error has been preserved; in many other cases the position of /credelitate/ led the scribe to see here a quality of the Serpent, and to make the change to /crudelitate/; some went further astray, as the readings /incredulitate/ and /cruditate/ testify; others managed to conjecture aright, /credulitate/. The fact that of the pair mp, which cohere so closely over the treatise, m has

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1. Stoll, op. cit. (see Thes., p. 2:11 n.) observed an error of this sort experimentally (p. 111). A subject who was asked to copy the sentence: "Als eine...Welle über das Verdeck hinschlug, das Fahrzeug...schleuderte" wrote "hinfuhr" instead of "hinschlug", under the influence of "Fahrzeug". Kane, op. cit. (see p. 2:13 n.) notes several instances that have arisen in mss of Piers Plowman (p. 122) such as "in lynene yclopid" → "in lynned yclopid" (I.3) and "ripe chiries" → "riche chiries" (VII. 278).
 2. for the treatise does not offer any evidence, in the form of errors common to the whole tradition, that all our extant mss go back to some ancestor or archetype later than the original.

/credulitate/ and p /crudelitate/, suggests that the latest common source on which they both drew still had /credelitate/, which was dealt with differently by these two descendants. The same can be said of the other closely-knit groups which are divided here.

214.23 One of Cyprian's best-known sentences is printed by Bévenot: "Habere iam non potest Deum patrem qui ecclesiam non habet matrem". All the members of our team, however, agree in omitting /iam/. The distribution of the two readings is shown in fig. B.6.4.



Before we discuss the respective merits of these readings, we must note a further piece of textual evidence. Fulgentius, bishop of Ruspe (467-533), quotes the first five sentences of Ch.6, including the present one, in his *De Remissione* i 21. This work has recently appeared in a critical edition¹, in which all the authorities (the

1. *Corpus Christianorum, Series Latina*, vol. XCI A, Turnhout 1968 (see p. 669).

oldest being a ms of cent. ix-x) lack /iam/.

Bévenot advocates (TM, pp. 68 f) that /iam/ be retained, in that its presence "fits more exactly the context of Cyprian's thought. He is preoccupied with schismatic movements, not with the fate of those who have never known the Church. It is those who are or were in the Church that he is addressing, and (with the reading /iam/) he is telling them that they can no longer expect to have God as their Father, if they do not look to the Church as their Mother". The absence of /iam/ in some mss can then be explained as an accidental omission.

It is possible, however, to argue in favour of the shorter reading. Without /iam/, this sentence has wider application, signifying that all who failed to recognise the Church as their Mother could not hope to claim God as their Father - whether they had deserted the Church (as in the immediate context here) or whether they had never entered it. Now the concept of the Motherhood of the Church is very frequent in Cyprian, and it is often linked with that of the Fatherhood of God¹. For example, in De Lapsis §9, he denounces those parents who brought their children with them when they obeyed the imperial order, during the persecution under Decius, to sacrifice to the pagan gods; those infants could plead "Nos nihil fecimus....illi nobis ecclesiam matrem, illi patrem Deum negaverunt..." We may therefore suppose that Cyprian formulated here a general proposition, which

1. J. C. Plumpe, "Mater Ecclesia", Washington 1943, discusses the matter in detail.

took in schismatics as a particular case. Similarly, the sentence which follows in the De Unitate ("Si potuit evadere quisque extra arcam Noe fuit, et qui extra ecclesiam foris fuerit evadet") can also be understood in general terms. In that case, the addition of /iam/ would place an unwanted restriction on Cyprian's sentiment.

We may perhaps discern confirmation that Cyprian had in mind this wider context, in another Cyprian passage which shows a striking resemblance to the present one, viz Ep. 74.7 (Hartel, 804.23 f). There Cyprian is deriding the baptism of heretics: "ubi et ex qua et cui natus est qui filius ecclesiae non est? ut habere quis possit Deum patrem, habeat ante ecclesiam matrem". Here again Cyprian declares that the Motherhood of the Church is a necessary pre-condition for the Fatherhood of God; but he applies it now to the individual who was born and baptised in heresy, and who, never having himself been within the Church, could not be said to have withdrawn from it¹.

If we hold that /iam/ is not original, we must explain how it ever originated. As Bévenot remarks, it could scarcely have been inserted deliberately. However, we may attribute its presence to scribal error. Let us suppose that

1. One might argue that heretics, viewed collectively, could indeed be said to have forsaken the Church, so that the application found in Ep. 74.7 is not really wider than that postulated by Bévenot in the De Unitate passage; but the context in Ep. 74.7 shews that Cyprian is speaking of heretics on an individual level.

a scribe was copying the text (without /iam/) from a ms which had about twenty-eight letters to the line and had the sentence laid out thus:

.....HABERE
 NON POTES DEUM PATREM QUI ECCLES
 IAM NON HABET MATREM

It is possible that when the copyist reached HABERE, his eye skipped to the beginning of the wrong line, so that he wrote IAM straight afterwards, and then resumed at the right place (NON POTES). Perhaps he did not notice his mistake; perhaps he marked the letters IAM for deletion but left them legible¹, and a later scribe copied them down; in either case, the origin of /iam/ can be explained. We shall have to suppose that this happened before cent. ix, to which some of the mss reading /iam/ are assigned.

A ms which illustrates two of the points involved in this hypothesis is described by R.A.B. Mynors². The ms is dated cent. vii/viii. One page is reproduced (Plate IV), containing the beginning of Mark. It was written in two columns, and shows a number of examples of a line containing exactly 28 letters (e.g. the fourth line of the second column).

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1. e.g. by putting dots above, within or below the letters; the Lindisfarne Gospels (c. 700) offer many examples of this practice.
 2. "Durham Cathedral Manuscripts to the end of the twelfth century", Oxford 1939.

There are moreover several instances of a word being divided between two lines, with three letters left for the second line (e.g. calciamento/rum, Simo/nem); sometimes these three letters, like /iam/, happen to form a real word (pi/lis, cu/ius).

It may be objected that a scribe who could not fit ECCLESIAM into the line would have had recourse to some abbreviation, rather than dividing it between two lines. Certainly the word has been abbreviated (to ecclm̄, for example) on occasion: W.M. Lindsay¹ lists several abbreviations, some perhaps as old as cent. vii, of /ecclesia/ and its cases. Some scribes, however, preferred to write the word out in full: it would seem that neither in the Lindisfarne Gospels (c.700)² nor in the John Rylands Cyprian³ (late eighth century) is /ecclesia/ (or its cases) ever abbreviated.

One may therefore imagine that /iam/ did arise in this way in some ancient ms, which was directly or indirectly drawn upon by those mss which preserve it today.

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1. "Notae Latinae", Cambridge 1915, p. 424. The book is concerned primarily with the early miniscule period (c. 700-850).
 2. A detailed account of Latin abbreviations employed in the Lindisfarne Gospels has been given by T.J. Brown in the magnificent two-volume edition of 1960 (vol. 2, pp. 70 f); it makes no mention of /ecclesia/.
 3. A. Souter's "List of Abbreviations and Contractions, etc., in John Rylands Latin Manuscript No. 15", Bulletin of the John Rylands Library, vol. 5 (1918-20), pp. 111-115, is similarly silent regarding /ecclesia/.

215.15 Christ's robe is referred to as "incorrup^ta adque indivisa tunica". For /indivisa/, there is a variant reading /individua/, which Bévenot was at first inclined to accept as superior on intrinsic considerations¹, but did not adopt for the final text when he became aware of "the close connexions between the chief MSS" that attest it (BkPWY, plus six others not included among our eighteen). However, the distributional evidence is not completely unfavourable to /individua/, which has the support of one member of the team (Y); and we may therefore wonder whether it should not after all be admitted to the text.

At 217.4 Cyprian sees an allusion to the necessity for Church unity in the instruction given to the harlot Rahab, that she should gather all her family into her house. The phrase that concerns us here is "cum dictum sit ei in qua praeformabatur ecclesia", after Bévenot's text. Now the reading /ei/ occurs in no more than a minority of the mss (Dhmp, plus seven others not included in the eighteen); the majority, including all four members of the team, read (with some variations of spelling) /ad Raab/. Bévenot argues (TM, pp. 70 ff) that /ei/ is original, for three reasons. First, Cyprian had portrayed, just a few hundred words previously, the purity of the bride of Christ ("Adulterari non potest sponsa Christi, incorrupta est et pudica..." 214.17); if he now had to refer to the harlot Rahab, he

1. TM, p. 69, to which the reader is referred for details of his argument.

would surely have preferred the obliquity of the pronoun to an explicit mention of her name. Second, it is easy to explain how /ad Raab/ could have arisen from /ei/: a scribe - or rather any number of scribes - could have inserted the name in the margin, and "its incorporation in place of /ei/ would follow as a matter of course at the next transcription"; but "it is highly improbable, to say the least, that a copyist or revisor should spontaneously have replaced /ad Rhaab/ by /ei/". Third, the mss which attest /ei/, albeit few in number, come from "three, apparently independent, traditions", among whose representatives are D, h and m-p respectively.

Yet a case can also be made out in favour of /ad Raab/. Let us examine Bévenot's three arguments in turn.

Concerning the likelihood of Cyprian's having used /ei/ in preference to /ad Raab/, two points deserve to be made. First, it is by no means certain that Cyprian would have thought it inappropriate to write /ad Raab/. We may compare the one other passage where Cyprian speaks of Rahab, in Ep. 69.4 (752.22). There the context is very similar to our own¹; and, having in Ch. 2 of the epistle applied to the Church the epithets "perfecta mea", "hortus conclusus" and "fons signatus" - all of them terms derived from the Song of Songs, to denote

1. In our paragraph from the De Unitate (§8), the unity of the Church is illustrated by Rahab's house and the Paschal lamb, and a citation of Ps. 68:6 follows; in Ep. 69.4, we find similarly, with reference to the same theme, a discussion on the Paschal lamb and Rahab's house, with a citation of Ps. 68:6 soon afterwards (§5 fin.). Thus there seems to be a close relationship between the two works at this point.

virginity - he does not shrink from mentioning Rahab by name: "quod item circa Raab quae ipsa quoque typum portabat ecclesiae expressum videmus..."¹ The second point is that the expression "ei in qua praeformabatur ecclesia" would not have been readily intelligible to Cyprian's hearers. It is to be rendered: "to her in whom the Church was pre-figured". Cyprian might have expected others to know who was meant if Rahab had been the only woman in the O.T. whom he regarded as a type of the Church, or at least the one who would have come most readily to mind; but as he saw such types in Rachel² and Hannah³ also, one may wonder whether "ei in qua praeformabatur ecclesia" was perspicuous enough an expression for Cyprian to have used⁴.

Nor can it be asserted as confidently as Bévenot does that the substitution, by a copyist, of /ei/ for /ad Raab/ would

1. This is the reading of all but one of the mss (some of which go back to cent. ix); we shall mention presently the one exception.
2. Test. I.20 (53.2): "sic et Iacob accepit uxores duas, ...minorem speciosam Rachel typum ecclesiae".
3. De Dom. Orat. §5 (269.11): "Anna in primo Regnorum libro ecclesiae typum portans".
4. Bévenot's rendering "to one in whom the church was pre-figured" would remove this difficulty; but such an indefinite usage of /is/ would be rare, if not anomalous, and finds no real parallel in the two-column article of Lewis and Short. The normal meanings of /is qui/ are "the one who..." and "the sort of man who would...."

have been "highly improbable". A copyist might indeed have felt sufficiently embarrassed by the harlot's name to replace it with a pronoun. This is not the only passage in which Rahab seems to have been the victim of scribal prudery. At Mat 1:5, where she has a place in the genealogy of Christ (Σαλμων δὲ ἐγέννησεν τὸν Βοες ἐκ τῆς Ραχαβ), Legg notes that three Greek mss omit the phrase in which she is mentioned (ἐκ τῆς Ραχαβ)¹; if the omission of Rahab's name is no more than an unintentional scribal oversight, then it is odd that no Greek ms is reported as having omitted the analogous phrases ἐκ τῆς θαμαρ / Πουθ / τοῦ Οὐρίου (VV 3/5/6)². Another instance may be discerned in the tradition of Cyprian himself, in a passage just mentioned (Ep. 69.4): while all but one of the mss read "quod item circa Raab quae ipsa quoque typum portabat ecclesiae expressum videmus", Hartel's apparatus indicates that S has: "quod item circa atque ipsa quoque typum..." So abashed was the scribe at the mention of the harlot that he produced a text that was hardly intelligible; and it is to such a mentality, rather than to Cyprian's own, that the somewhat evasive reading "ei in qua praeformabatur ecclesia" may be fittingly ascribed.

1. See S.C.E. Legg's edition and apparatus of Matthew (Oxford 1940), which gives the most copious collection of ms variants yet published.
2. It is only fair to mention, however, that the phrase καὶ τὸν Ζαρα ἐκ τῆς θαμαρ (V.3) is lacking in Epiphanius and in two Old Latin mss; ἐκ τῆς Πουθ in one Coptic ms and in Epiphanius; ἐκ τῆς τοῦ Οὐρίου in one Vulgate ms. None of these omissions, one presumes, was intentional.

Finally we come to the argument that D, h and m-p represent three "apparently independent" traditions. Now our work so far has amply shewn that "independent" is very much a relative term, and that the question of the connection that exists among a given group of mss is one of great complexity. True, there are no obvious grounds for supposing that the source-complexes of D, h and m-p had much in common; but the possibility remains that the reading /ei/ has reached these four mss from a source other than the original, while the majority of the mss preserve the true reading.

218.20 The opening words of a quotation of 1 Cor 11:19 are given by Bévenot as: "Oportet haeresis esse". The text suggested by the team differs in two respects: "Oportet et hereses esse".

Let us deal first with the question of /et/. Most of our Greek mss have δεῖ γὰρ καὶ αἱρέσεις ἐν ὑμῖν εἶναι, and none¹ apparently omits καὶ. The Vulgate text is usually given as: "nam oportet et hereses esse". This constitutes an argument in favour of Bévenot's choice of reading; when we are confronted with two alternative readings in a scriptural text quoted by Cyprian, we should prefer the one which less resembles the Vulgate, on the grounds that its rival may be presumed to be due to assimilation to the Vulgate text.

1. according to Tischendorf's apparatus.

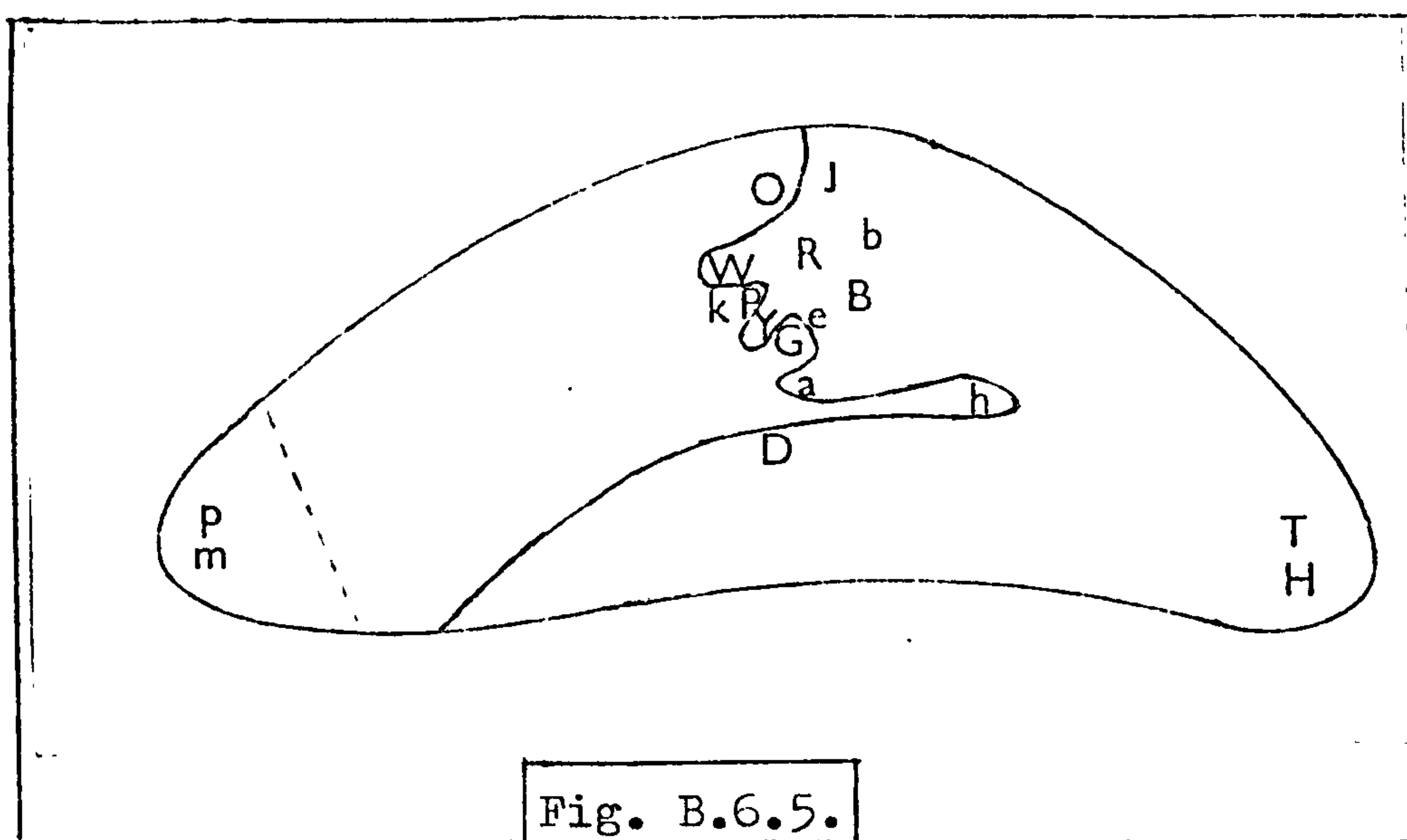
In this particular case, however, there may be grounds for departing from that principle. First, if we suppose /oportet et/ to be original, then the omission of /et/ can be easily explained by haplography. Second, Wordsworth and White show that the Vulgate tradition is itself divided, by reporting in their edition about a dozen witnesses which lack /et/. This fact demonstrates that the omission of /et/, through scribal error, did actually occur in this passage; it also renders double-edged the argument concerning accommodation to the Vulgate, for we can now state that not only the presence of /et/ but also its absence could be attributed to assimilation to some manuscript of the Vulgate text. On this question, then, the conclusion to which our team led us can be upheld.

Cyprian cites the verse once more in the Testimonia (or Ad Quirinum), of which a critical edition is now available (Corpus Christianorum, Series Latina, vol. III). That quotation (Test. III, 93) does not, however, offer any direct help for deciding on /et/ in our own passage, because there too the mss cited are evenly divided (nine against nine) on this very question.

The choice between /haeresis/ and /hereses/ is a rather different matter. If Bévenot is correct in believing /haeresis/ to be the form which Cyprian used for the acc. pl., then it is likely that each of the scribes of the team mss, in writing /hereses/, has followed his own habits of orthography and thereby lost the original form. Thus /haeresis/ should be read, despite the agreement of the team to the contrary. Note that the team elsewhere does offer some

support for the ending -is in the acc. pl. (RY, 211.16) and also in the nom. pl. (GR, 218.14).

The next passage for discussion, at 219.14, also falls within a scriptural quotation. Those who propagate heresy are likened to the false prophets against whom Jeremiah complained (23:16-21). According to Bévenot's text, the Lord, speaking through Jeremiah, declares: "Si stetissent in substantia mea....., convertissent eos a malis cogitationibus eorum". Though most mss (including eRY) have 3 pl. /convertissent/, some have 1 sing. (convertissem: G+hkPO; converterem mp). The two readings divide the map fairly evenly (fig. B.6.5); and choosing between them is, as we shall see, a very delicate matter.



If we look beyond this one citation of Jer 23:22, we find that the Latin tradition is almost unanimously in favour of 1 sing. The Vetus Latina of P. Sabatier lists

five citations of this version¹, all of which have /avertissem/ or (in one case) /averterem/. Of one of the works concerned, viz the De Fide of Ambrose (c. 339-397), a critical edition has appeared, which confirms the reading /avertissem/ and mentions no trace of a 3 pl. form in the apparatus². We may add a citation in the De Divinis Scripturis, or Speculum, of Augustine; there a critical edition gives the reading /avertissem/, without reporting any variation³. I have encountered just one piece of Old Latin evidence in favour of 3 pl.: the Würzburg palimpsest⁴, which contains sundry fragments in this version, has at this point /convertissent/. In the Vulgate too, the 1 sing. form (avertissem) can be seen from the great Roman edition to be overwhelmingly predominant - though one ms (Ω^J, written ca. 1250) has /avertissent/, which the "Correction" attributed to Hugh of St Cher also observes to be the sense of the Hebrew.

Why has the 1 sing. form become so widespread in the Latin tradition, while the Hebrew (wîšîhûm) and the other ancient versions agree in having 3 pl.? The probable explanation is suggested by W. Rudolph⁵: the LXX rendering

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1. They may be conveniently recognised by the phrase "in substantia mea" (LXX ἐν τῇ ὑποστάσει μου); Vulg. has /in consilio meo/, Hebrew bēsôdî.
 2. CSEL lxxviii, p. 150.
 3. CSEL xii, p. 631.
 4. E. Ranke, "Par Palimpsestorum Wirceburgensium", Vienna 1871.
 5. Biblia Hebraica, ed. Kittel, 3rd edition, note ad loc.

ἀπείστρεφον was doubtless intended as 3 pl., but was popularly construed as 1 sing. Thus a 1 sing. rendering entrenched itself in the Old Latin tradition, to such an extent that Jerome, who had consulted the Hebrew, did not eradicate it in his own version in favour of a 3 pl. form.

Two possibilities are therefore open with regard to Cyprian's text. Either /convertissent/ is original. In that case Cyprian adds his authority to the little-attested 3 pl. form in the Old Latin; and the rival readings /convertissem/ and /converterem/ are due to the influence of the Vulgate. Or Cyprian wrote what appears to have been the far more widespread 1 sing. form (/convertissem/ or /converterem/¹), and /convertissent/ is due to scribal error - perhaps of perseveration, in view of the sequence of verbs immediately preceding (stetissent...audissent...docuissent). The former possibility, which commended itself to Bévenot, is supported by three team members, the latter by one alone; but I think it difficult to choose between the two with any confidence.

The last passage selected for discussion is at 233.9. It falls within the last Chapter of the treatise, which is reproduced below according to Bévenot's text.

1. We may suppose that the former is original and the latter due to a modification on the part of a copyist.

Excitemus nos quantum possumus, dilectissimi fratres, et somno inertiae veteris abrupto ad obseruanda et gerenda Domini praecepta uigilemus. Simus tales quales esse nos ipse praecepit, dicens: 'Sint lumbi uestri adcincti et lucernae ardentes, et uos similes hominibus expectantibus dominum suum quando ueniat a nuptiis ut, cum uenerit et pulsauerit, aperiant ei. Beati serui illi quos adueniens Dominus inuenerit uigilantes. Adcinctos nos esse oportet ne, cum expeditionis dies uenerit, inpeditos et implicitos adprehendat. Luceat in bonis operibus nostrum lumen et fulgeat, ut ipse nos ad lucem claritatis aeternae de hac saeculi nocte perducatur. Expectemus solliciti semper et cauti aduentum Domini repentinum ut, quando ille pulsauerit, euigilet fides nostra, uigilantiae praemium de Domino receptura. Si haec mandata seruentur, si haec monita et praecepta teneantur, opprimi dormientes diabolo fallente non possumus: serui uigiles Christo dominante regnabimus.

The point which concerns us is that the reading /ipse/ (line 11) is found in hHmpT alone. R has the nonsense word /ipsut/; and the rest of the mss, including the three team members eGY, read /ipsum/.

Both /ipse/ and /ipsum/ yield satisfactory sense. According to the former, the subject of /perducatur/ is Christ; with this reading, Bévenot translates¹ the whole sentence: "Let our light shine brightly in good works, so that He may Himself lead us from the darkness of this world into the splendour of eternal light". On the other hand, /ipsum/ indicates that it is the light of our good works that will render manifest the path to salvation. In E.H. Blakeney's version²: "Let our light shine out in good works, and so gleam as to guide us from the night of this age unto the light of eternal day".

1. in his Latin and English edition, Oxford 1971.

2. London 1928.

As we have noted, Bévenot prefers the reading /ipse/, which is not found among any of our team mss. Now the distributional evidence tends, on the whole, to support /ipsum/ (eGY and others). It is not, however, completely unfavourable to /ipse/, which may have the support of one member of the team (R) in a disguised form; for the reading /ipsut/ of R, coming after /ut/, is evidently due to perseveration, and the reading of that source of R which was being copied when this error was made might have been not /ipsum/ but /ipse/. Thus distributional considerations favour /ipsum/, but not quite unequivocally.

When intrinsic criteria are considered, there is much to be said for either reading. In favour of /ipse/ we may state, first of all, that the verb /perducat/ goes more naturally with a personal than with an inanimate subject. Moreover, there is nothing unexpected in the use of the pronoun /ipse/ to refer to Christ; but if we read /ipsum/, the emphasis thereby laid on /nostrum lumen/ (to which /ipsum/ will of course refer), which has already received prominence through being the subject of the main clause, seems indeed excessive. Bévenot further argues in favour of /ipse/ (TM, p. 58) that "it makes Christ himself our light", and that light is one of Cyprian's favourite metaphors applied to Christ. This last point does not necessarily strengthen the case in favour of /ipse/: true, we thus obtain a further allusion to light, but the relationship between the three lights that result becomes somewhat complicated, with the light of our good deeds (1) moving the light that is Christ (2)

to lead us to eternal light (3). But at all events, whether or not we see in /ipse/ a reference to Christ as light, the merits of /ipse/ cannot be denied.

On the other hand, one could argue in favour of /ipsum/ that it fits more precisely the tone of watchfulness and urgency which pervades the Chapter. Cyprian is telling his hearers that they cannot take it for granted that they will find favour before Christ and merit salvation; they must prove themselves through their own works, and, as they now stand, they are far from being assured of victory in the battle that is to come. The text with reading /ipsum/, by focusing on the part played by our own efforts (rather than that of Christ) in our attaining salvation, would surely encourage Cyprian's readers all the more to observe the Lord's commandments¹.

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1. Another feature of Blakeney's translation deserves to be pointed out here. Many translators have taken the subject of /adprehendat/ (see line 10) to be /expeditionis dies/; thus Bévenot renders: "Our loins must be girt, lest when the day comes for the campaign, it find us encumbered with trappings". Blakeney, however, makes Christ the subject: "Needs be that we should be girded, lest, when the day of warfare cometh, he find us hindered and unready". The latter translation follows on well, as a fine antithesis, from the sentence that precedes; and it deserves consideration because it reinforces the impression of urgency by presenting the more distressing prospect of not only knowing oneself to be unworthy but also being found to be so by Christ.

The sentiment that good works rebound to the advantage of the doer and can secure his salvation, fits in with many of Cyprian's statements in his treatise "De opere et eleemosynis". There, for example, Peter's revival of Tabitha is cited as proof "quod eleemosynis non tantum a secunda sed a prima morte animae liberentur" (377.21 f); through the practice of charity, "perpetuum praemium regni caelestis accipitur" (389.28 f). As for the objection that /ipsum/ is unduly emphatic, two points may be made. First, /ipse/ does not always have the same emphatic force in Cyprian's usage as in classical Latin; J. Schrijnen and C. Mohrmann point out¹ that Cyprian sometimes uses /ipse/ where an earlier author would merely have written /is/. Second, Cyprian may have sought, by the use of the pronoun, to draw attention to the new aspect which he had imparted to the parable quoted from Luke. The light of good works is assigned, in the parable itself, no more than a passive role, as it waits to be seen by the Bridegroom when he returns; Cyprian, however, if the reading /ipsum/ is adopted, portrays the light as actively guiding one towards salvation. If these two points are borne in mind, the reading /ipsum/ can be justified; /ipse/ is then perhaps due to the influence of "Simus tales quales esse nos ipse praecepit" (lines 3f. above).

Thus we have here one more case in which the mss. are divided between two attractive readings, and there seem to be

1. "Studien zur Syntax der Briefe des hl. Cyprian", Nijmegen 1936, Erster Teil, p. 155.

no absolutely conclusive grounds for adopting either in preference to the other.

We have left to the end the question of the title of the treatise, which Bévenot takes to be "De ecclesiae catholicae unitate". None of the members of the team, however, gives the title in precisely this form. e reads /de unit. eccl./ in the title, and has no subscription at the end of the treatise; G has /de eccl. unit./ in both title and subscription; R has /de unit. eccl./ at both points; Y reads /de cath. eccl. unit./ in the title, but /de eccl. unit./ in the subscription. One hesitates to judge a title by the criteria appropriate to passages in the text itself; but it is remarkable that team support for the presence of /catholicae/ is confined to Y, and that even Y omits the word in the subscription. This evidence tends to support the view of H. Koch¹, who maintains, partially on the grounds that the expression 'catholica ecclesia' does not occur in the treatise, "dass auch der Titel nur, de ecclesiae unitate⁶ oder, de unitate ecclesiae⁶ gelautet haben wird".

It should be made clear, at the close of the discussion, that I have not regarded it as a matter of inviolate principle to defend all readings, whatever their intrinsic merits, that enjoy the support of the four-manuscript team eGRY. It has

1. "Cyprianische Untersuchungen", Bonn 1926, pp. 102 ff.

simply turned out that in almost every case in which the team offers no support for the reading which Bévenot selected, intrinsic considerations emerged which shewed that the question of deciding between the rival readings concerned deserved to be re-opened.

B. AESCHYLUS: PERSAE

References

- A. Turyn, "The Manuscript Tradition of the Tragedies of Aeschylus", New York 1943.
 R.D. Dawe, "The Collation and Investigation of Manuscripts of Aeschylus", Cambridge 1964.

Our second preliminary study relates to the Persae of Aeschylus, first acted in 472 B.C. Some 150 mss containing this work are known to exist; the present study however is limited to the sixteen which have been collated by Dawe.

We shall depart from the procedure which we followed in the case of the De Unitate, in two respects. First, there will be no detailed discussion of rival readings; that would, I fear, lie beyond my competence. Second, there are presented here, besides the now familiar two-dimensional map based on straightforward resemblance in text, two further maps: one in three dimensions, based on the same criterion, and one in two dimensions, based on the criterion of agreement in error.

The sixteen mss with which we shall be concerned are listed in Table B.6.2. Each is followed by the century to which it is assigned in Turyn's book. It need hardly be pointed out that the time interval between the original and the mss is far greater here than in the case of the Cyprian text.

On the basis of the information available to him, Turyn attempted provisionally to formulate the inter-relations of the mss of Aeschylus in stemmatic terms. In the course

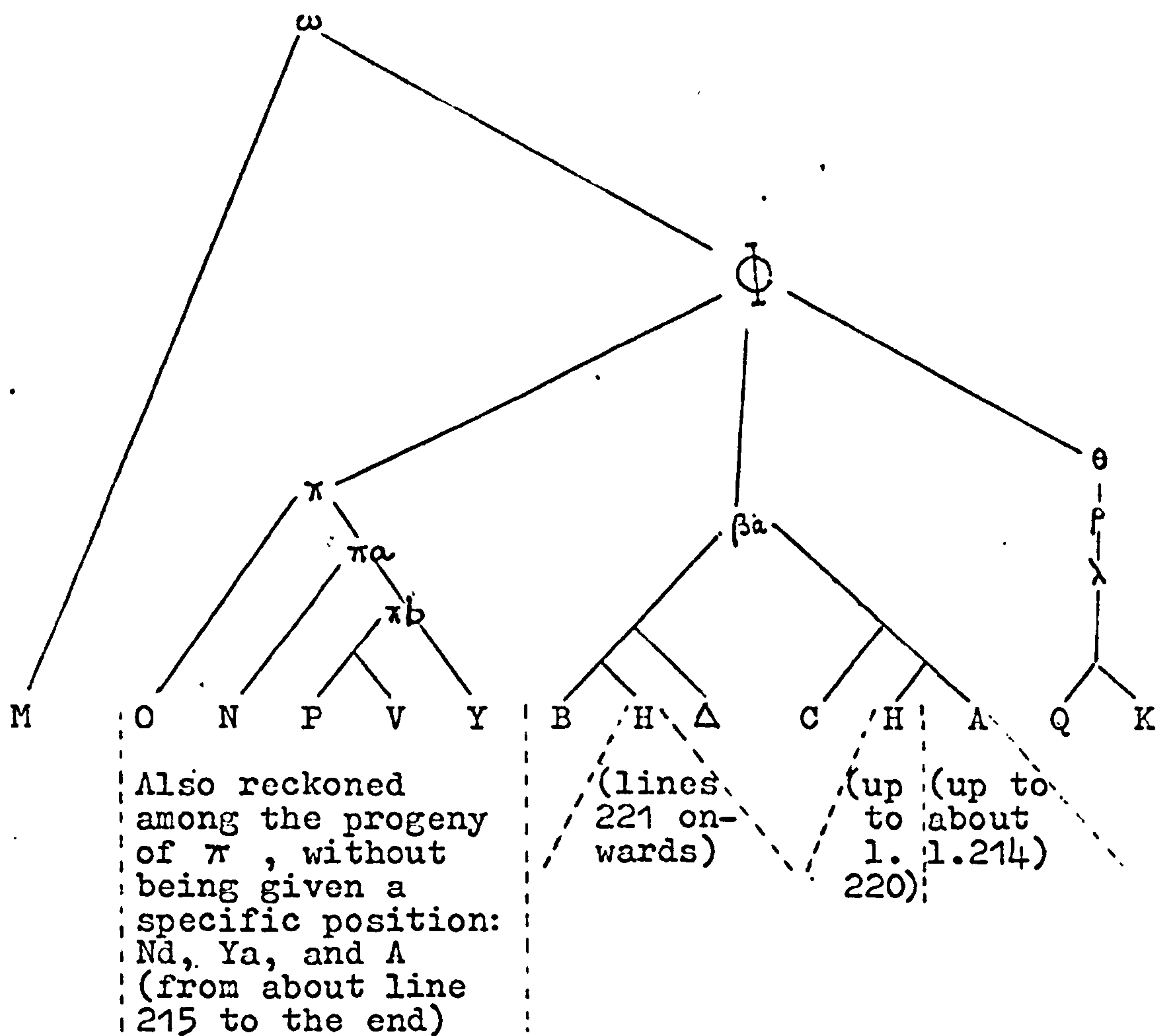
TABLE B.6.2

manuscript	A	B	C	Δ	H	I	K
century	xiv	late xiii	xiv	xiv	xiv	early xiv	xiv

manuscript	M	N	Nd	O	P	Q	V	Y	Ya
century	x or early xi	xiv	xv	xiv	xiv	late xiii	xiii	xiv	xv

of his investigation, he studied almost all the mss that are known to scholars; of our sixteen, the only one which he did not investigate was Cod. I. The amount of text on which he grounded his conclusions, however, was rather modest; Dawe observes (p. 4) that "most of the material used was of the first few hundred lines of the Prometheus". The relationships which Turyn postulated between our mss (apart from I), with particular reference to the text of the Persae, can be extracted from statements made at various points in his book, and are shown in fig. B.6.6.

Dawe, however, having fully collated these sixteen mss over the Byzantine triad, shewed not only that Turyn's stemmatic representation failed to account for the texts actually presented by the mss concerned, but also that one could not hope to arrange the mss in the form of a stemma at all. He found, inter alia, (1) that the patterns of agreement and disagreement among the mss at different points in the text showed little order or regularity and were far



Note. The symbols which are not mentioned in Table B.6.2 (ω , Φ , β_a , etc.) represent lost common ancestors whose existence is postulated by Turyn; they have been retained here in order to facilitate comparison with the stemmata of Turyn from which this stemma has been compiled (see pp. 16, 32, 34, 53, 80, 115).

Fig. B.6.6

from being stemmatically consistent¹, and (2) that of nearly every one of the sixteen mss it could be maintained that passages exist wherein that ms is the only one to preserve the original reading; neither phenomenon can be reconciled with any possible stemma. Unlike Bévenot, Dawe did not propose any alternative approach for utilising the distributional evidence; he preferred to evaluate rival readings primarily by attention to their intrinsic merits, and his book does not suggest that his judgment was greatly influenced by the question of which mss lent support to the respective readings. As Prof. D. Page observes in his preface to the third edition (1972) of the Oxford Classical Text, in which Dawe's work has been fully taken into account: "non stemmate...sed virtute in unoquoque codice est unaquaque lectio iudicanda" (p. viii). One cannot but admire the critical acumen which shows itself in Dawe's discussions of such intrinsic considerations; yet the extent to which scholars disagree regarding so many of these points demonstrates how difficult it is, in many cases, to reach a decision with reasonable confidence, let alone certainty. The possibility that the distributional evidence can somehow be brought to bear on these problems, surely deserves to be followed up; and the present study represents a tentative essay in that direction.

1. We have ourselves gone into the matter in detail (pp. 2:45 ff).

The text from which our data has been drawn goes up to line 746 (since we do not have the readings of the first hand of N beyond that point). Details of our policy in admitting variants are given in Thes., p. 2:45, n.3. The number of variants yielded by this stretch of text was 404. Hence percentage distances¹ were calculated as before, and from these was formulated the map shown in fig. B.6.7. The "stress" was found to be 13.7, which is the second highest value obtained for any of our maps; in other words, this 2-D map provides a less satisfactory fit than most of the others.

Most of the small groups which Dawe detected among the mss are illustrated by this map. One can readily discern the clusters BHC Δ , OYYa (within which O and Y are particularly close), PVNNd and QK. The fact that C, while belonging to the BH.... group basically, has definite leanings towards the group PVNNd, is also brought out. The most notable point in Dawe's classification that finds no confirmation in the map, is that he held that Δ had "marked connections" with M and I (p. 28), despite its primary affinity with BHC; but in our section of the Persae, there seems in fact to be little justification for

1. The problem of mss being deficient for one reason or another is rather more serious here than in the case of Cyprian.

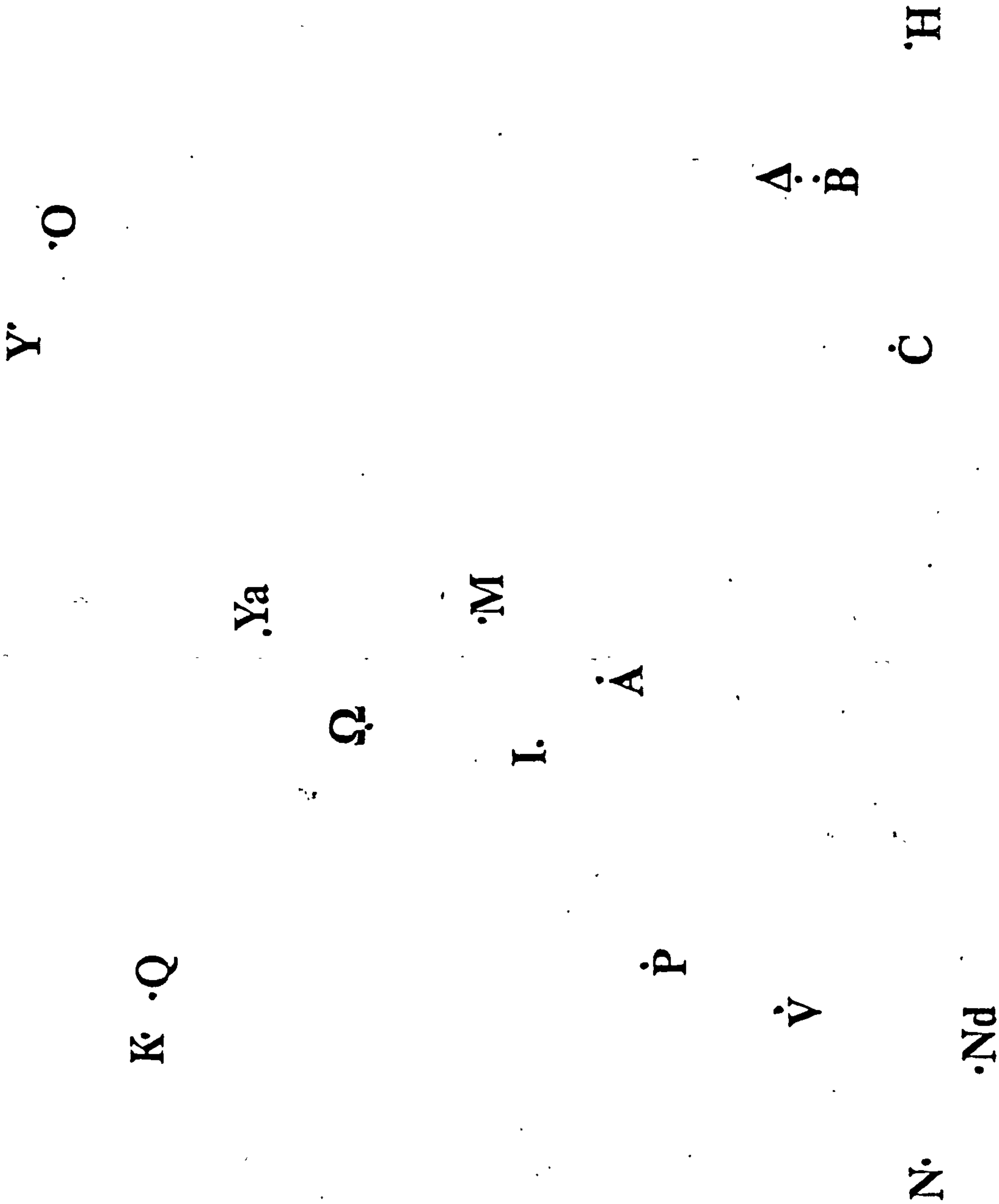


Fig. B.6.7

singling out Δ as having particular affinity with MI^1 .

The location of Ω was accomplished in much the same way as for the De Unitate, and is subject to the same reservations. The 'provisional Urtext' which served as the starting-point for providing a tentative estimate of the number of errors in each ms, was taken to be the second edition (1955) of the O.C.T. (against which Dawe made his collations), except that I have followed Dawe on those occasions when he prefers a different reading².

The reading thus selected, at any given point in the text, is hereinafter termed the approved reading. Table B.6.3 shows the number of times each ms diverges from this text, absolutely and as a percentage of the number of passages (out of the 404) in which it is available.

1. The passages adduced by Dawe are: 49) στεῦται ΔM^1 pc, στεῦνται most others; 160) κάμοῦ ΔI , κάμδον rell.; 164) omitted in ΔI ; 245) τεχοῦσιν ΔM , τεχοῦσι most others; 316) δάσχειον ΔM , δάσχιον most others; 645) οὕτω $M^{ac} \Delta$, οὕτω most others. In all but the third of these passages, the variation concerns very minor matters, and the agreement could easily be coincidental. One might just as well postulate a special relationship between MI and Y - to take one of several possible examples - on the basis of 243) ἄν οὕν: om. οὕν IY ; 412) μὲν νῦν MY , others μὲν οὕν or μὲν δῆ, etc.; 730) κενανδρίαν: κενανδρία IY (compare κενανδρία, followed by an erasure of one letter, in M).
2. The resulting text is close to the third edition of the O.C.T.

TABLE B.6.3

manuscript	A	B	C	Δ	H	I	K	M	N
number of errors	89	134	124	136	149	75	102	43	148
percentage	23	37	34	35	40	19	26	11	38

manuscript	Nd	O	P	Q	V	Y	Ya
number of errors	154	132	105	85	132	138	109
percentage	39	34	27	23	34	37	28

Interpreting the percentage figures as percentage distances between Ω and the several mss, we may locate Ω as a fragment; its position is shown in our map.

The choice of a team of mss which "comfortably" surround Ω is straightforward; we select the four mss IMQYa. Thus we arrive, for the third time, at a team consisting of four mss; the reason seems to be that, in two dimensions, we often cannot accommodate Ω "comfortably" in any triangle of ms points, but encounter little difficulty in setting up a four-sided figure that will suffice. This team conflicts in some interesting respects with Turyn's view (p. 116) that "the codices MNVXBHC present an adequate manuscript basis for the triad"¹. The only ms that appears in both Turyn's "team"

1. Collations of X are not presented by Dawe; according to Turyn, X is a member of the π class, its closest relative among our sixteen being Y.

and our own is the famous Medicean codex M. Of the text of I (let alone its affinities), little was known before Dawe's study. Ya is our sole representative - and one of the latest (cent. xv) - of the large collection of mss which Turyn derives from a common ancestor $\bar{\Phi}$, and which provided all the mss (apart from M) which he selected for detailed study. Q he regarded as a member of the Thoman family (i.e. the recension of Thomas Magister, accomplished towards the end of the thirteenth century), which "must disappear from the critical apparatuses of future editions" (p. 115).

The performance of this team can be assessed as before. Over the total of 404 variant passages, the approved reading

- (a) is supported by the whole team (or by as many members as are available), 199 times;
- (b) enjoys majority support 124 times;
- (c) is found in two members of the team, while the other two agree in a rival reading, 37 times¹;
- (d) appears in only one ms of the team, 34 times;
- (e) has no support from the team at all, 10 times.

These results deserve to be amplified somewhat. They are, perhaps, not quite as encouraging as those for the De Unitate: the proportion of cases in which the approved reading has at least majority support from the team, is now only 80% (compared with 89% before). Regarding the 34 places where the approved reading is attested by only one ms of the team, that ms is I four times, M fourteen times, Q eleven times, and Ya five times.

1. They divide themselves IM:QYa seventeen times, IYa:MQ twelve times, and IQ:MYa eight times.

The ten places at which the approved reading does not occur in the team at all are of particular interest. They may be broken down into three categories:

I. Five passages in which the mss offer a number of alternative readings, but the approved reading is found in none of them and had to be supplied through conjecture. In all five, the team mss are themselves divided between such rival readings. The passages are listed below; the alternative extant readings are placed in brackets after the approved reading and separated by slashes.

128 μελισσῶν (μέλισσα / μελισσῶν / μελισσῶν)

270 ἦλθετ' αἶαι (ἦλθ' ἐπ' αἶαν / ἦλθεν ...) . Dowe notes that Q^c has ἦλθ' ἐπ' αἶαν, the π having been corrected from τ by the first hand; does this indicate that what Q first wrote preserves a vestige of the approved reading?

329 ὦν (νῦν / omitted altogether)

532 νῦν γὰρ Περσῶν (νῦν περσῶν / ΟΥ νῦν τῶν περσῶν / Q νῦν *** περσῶν, which might possibly conceal this approved reading).

675 δις γοέδν' (διάγοιεν / διάγοιεν δὲ)

We may presume that in all these cases the true reading became lost well before the time of our mss.

II. 2 places in which the approved reading, on which all three editions of O.C.T. and Dawe are agreed, appears in at least one extant ms but not within the team. In both, the team mss proved to be in disagreement among themselves:

107 τ'] So K, and B (as corrected by first hand); τε Y; I (with some others) omits; the remainder (including MQ¹Ya) have δ'.

218 τὰ δ' ἀγαθ'] The nearest reading actually found in a ms is O's τὰ δ' ἀγαθῶν . The other mss add a particle at the end of the phrase (apart from sundry variations within it): γ' in Q, δ' in most others (including IMYa).

Both variations lie in very minor matters, which can easily become occasions for error. The incorrect readings appear to be the result of tinkering with the text; one or two mss, however, have not been affected.

III. 3 variations in which Dawe, departing from the first and second editions of O.C.T., prefers the reading of OY alone to that of the majority (pp. 122 ff):

428 κελαινὸν OY, κελαινῆς rel.

652 πῶποτ' OY, ποτ' rel.

716 τις ὥχετο OY, κατέφθαρται rel.

Dawe argues most cogently in favour of these readings; the first two have been adopted in the latest edition of O.C.T. (1972), but not - somewhat surprisingly - the third. The topic cannot however be pursued here; to explain properly these discrepancies between Dawe's conclusion and our method, through discussion and evaluation of the rival readings, would lie well beyond my own prowess.

The suspicion arises that we have chosen the wrong member of the family OYYa for membership of the team. If we had selected O instead of Ya, the last four approved readings to which we have referred (lines 218, 428, 652, 716) would have had the support of one team member. Nor do we lose anything by discarding Ya; all the five readings which appear in Ya but in no other member of the team, are in O as well, namely:

17 Κίονον 115 μοι 266 γε 310 κυκώμενοι 598 ἔμπειρος

Admittedly, this line of argument has its drawbacks. The criteria which we have worked out for choosing a team on the basis of the locations of the different mss on the map, point clearly to Ya rather than O; and to juggle with these criteria in such a way is to risk cutting from under one's feet the ground of logical rigour and validity. In this particular case, however, there may be some excuse, in that the position of O on our two-dimensional map might not be completely reliable, as we shall see below.

Let us now turn to the two other maps which we have derived from our Aeschylus data.

The three-dimensional map gives a stress of 6.0, which represents a marked improvement on the two-dimensional map of fig. B.6.7; none of the other traditions that we have investigated shows so great a fall in stress when one switches from two dimensions to three. The map is shown in fig. B.6.8. The method whereby the impression of three dimensions is conveyed on paper was devised by Mr. D.T. Codd:

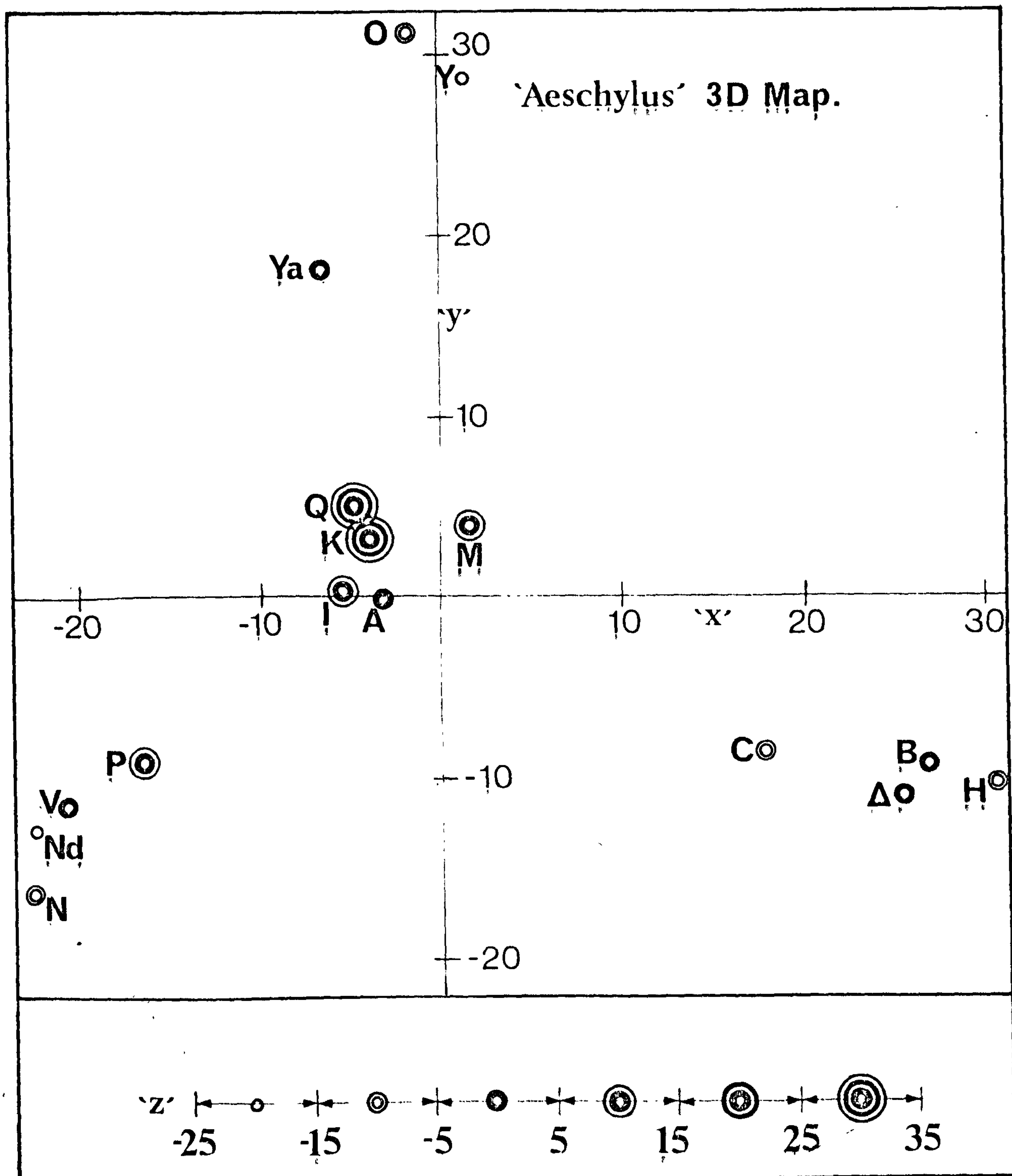


Fig. B.6.8

each point is represented by a "bull's-eye" target, and the further away that point must be imagined to lie from the observer, the smaller the target is drawn.

Thus the z -axis is perpendicular to the paper. The observer must be thought of as standing about thirty-five units along the z -axis (in the positive direction), and looking towards the origin. The lower the z -coördinate of a given point, the farther it will be from him, and the smaller it will appear.

It is interesting to compare this map with fig. B.6.7. In the 3-D map, the range of variation in the z -dimension is hardly smaller than in the y -dimension: the extreme z -values are those of Q(+30.1) and Y(-17.8). With the 'squashing' of this configuration into two dimensions, the mss with the largest z -values have been the most noticeably affected: QK now stand further away from the centre, in comparison with the other mss, and OY have moved from "north" to "north-east"¹. In general, however, the resemblance between the 2-D and 3-D maps cannot be mistaken.

It might be rewarding to locate Ω in the 3-D map, and to form a team by identifying a collection of points which are the vertices of the smallest polyhedron to contain Ω "comfortably". We shall need, however, to define, in unequivocal terms, just how this criterion is to be interpreted and applied; in particular,

1. Hence the possibility raised above, that O, being as it were refractory to a 2-D representation, has been forced into a position which does not reflect its relation to the other mss particularly well.

it would be desirable to have a computer programme to select an appropriate team, when one is given the co-ordinates of all the mss and of Ω . If such a procedure, having been devised, could be generalised to higher dimensions, then we should be able to set up a team for any textual tradition, no matter how many dimensions were required to draw a map with a satisfactory "fit". In view of what we have observed of the tendency for a 2-D map to yield a team of four mss, we may surmise that the higher the number of dimensions, the larger the team will be: a map in t dimensions will tend to give rise to a team of $(t+2)$ mss. Such an extension of our method, however, would go beyond the scope of the present study.

The other map which we have formulated from the Aeschylus data is based on the criterion of agreement in error. For each possible pair of mss a coefficient was calculated, being the number of places in which the two mss agree in an unapproved reading, expressed as a percentage of the number of places (out of the 404) in which both mss are available. The map is shown as fig. B.6.9. Certain familiar clusters can be distinguished (BCH Δ , NN Δ PV, KQ, OY), but its general appearance is quite different from that of our two previous maps, since it is based on a different criterion. For example, M lay near to the centre of our earlier maps, and had some reasonably close neighbours (AIYa) - for it differs but rarely from the approved text, and is quite similar to other mss whose text is fairly pure; but in fig. B.6.9, M stands well away

Q

K

2

i

IV

Y.

H

Δ.

3
U

BY

A

PZ.

7. Z.

7.

Fig. B.6.9

from all the other mss, because the occasions on which it agrees with any of them in error are relatively few¹.

A large cluster is formed by the twelve mss ABC Δ HNNdOPVYYa, while M, I and KQ stand apart, in various directions. It seems that the twelve mss are linked by a network of common errors - though the efforts of scribes to recover the correct reading have meant that comparatively few errors are common to all twelve. The obvious explanation is that they owe these errors to a common source, which has contributed little or nothing to IKMQ. An alternative hypothesis is that in most quarters the text degenerated seriously between cent. x and cent. xiii, spurious readings being propagated by scribes who were more industrious than discriminating; most of the mss bear the marks of this process; of those which have suffered little or not at all, IKQ owe the fact to the utilisation of sources which most copyists did not have the opportunity (or perhaps the inclination) to consult, while M was written before this degeneration got under way. But this is no more than a hypothesis compatible with certain observed facts; little can be asserted regarding the history of the text with any confidence.

1. One might perhaps say that the approximate relationship between the two 2-D maps is one of geometrical inversion with respect to Ω .

The fact that distance on this map can serve as an estimate of historical connection is relevant to the problem of selecting a team. The members of our original team, IMQYa, certainly are all distant from one another; but we can do better by substituting O for Ya, obtaining a quadrilateral which covers almost the whole map area. This tallies with our earlier observation, that the performance of the team could apparently be improved if Ya were replaced by O. Nevertheless, one could not advocate that, as a general practice, the team should be identified from a map based on community in error, rather than textual similarity; for in that case the whole map, and not just the location of Ω , would depend on the judgment of the investigator in deciding between alternative readings, and would be without value to another investigator who disagreed at all frequently with those decisions.

C. THE VULGATE ISAIAH

References

Biblia Sacra iuxta Latinam Vulgatam Versionem: vol xiii (Rome 1969): Liber Isaiae.

R. Loewe, "The Mediaeval History of the Latin Vulgate", in the Cambridge History of the Bible, vol. 2 (Cambridge 1969), pp. 102-154.

Among those who have been adventurous enough to consider a statistical treatment of a textual tradition, are the Benedictine editors of the great Rome Vulgate. In the volume referred to above, two numerical tables are presented (p. xxx). The former is a table of 'absolute distances' (entitled "lectiones discordantes") for a collection of 28 mss, dating from various times between ca. 700 and ca. 1270. The figures are based¹ not on the whole book but on the 219 variant passages which could be found in Chapters xxx, xxxi, xlvii, xlix:1-6, lxi:1 - lxiii:6. The latter table shows how often each ms departs, and how often each pair of mss agree in departing, from what is supposed to be the original reading. It omits four of the 28 mss (namely TUZΓ^A) and has been compiled from a different field of variants, namely the 120 which appeared in Chapters v, vi, xxv, xxvi, xlv. It was of course the former table that was used as data for our map, without any independent reference to the apparatus on my part. The result was fig. B.6.10, which shows all

1. For details of the editors' policy in admitting variants, see p. xxix, and also Thes., p. 4:14.

28 mss. Twenty-eight is the largest number for which we have yet attempted to formulate a map; it is probably no coincidence that we observe here our greatest value for the stress (16%).

Let us consider in the light of this map the classification proposed in the prolegomena to the Rome edition. The material used to reach that classification was not of course confined to the one table which we have proposed to utilise: the editors state that they found the second table rather more illuminating (p. xxxiii), and they also possessed a considerable body of textual evidence, both external and internal. In the upshot, they distinguished (pp. xv, xxxii ff, xlix ff) the following groups:

(a) $CX\Sigma^T \Delta^B \Delta^L$ form a Spanish family, described, in comparison with most of the other groups, as "fusior atque solutior" (p. xv).

(b) The two mss $\Delta^M \wedge^L$ are termed Visigothic, and have no particular links with any other family; "locum medium, ut ita dicamus, in traditione obtinent" (p. xxxv).

(c) $AOQ \Gamma^A \Pi^L$ are classed together as an Italian family.

(d) RSTUZ form a Transalpine family, whose constituent mss were written in eastern Gaul or Germany.

(e) The four mss $\Theta^A \Theta^G \Theta^H \Theta^M$, all dated ca. 800, represent the recension of Theodulf, bishop of Orleans (†821).

(f) The three ninth-century mss $\Phi^G \Phi^P \Phi^R$ belong to the recension of Alcuin ($\dagger 804$). Ψ^D is said on p. xv to follow this group, but on p. xl we are told that Ψ^D also has marked connections with the Theodulfian and Italian families.

(g) The three thirteenth-century mss $\Omega^J \Omega^M \Omega^S$ represent the text associated with the University of Paris.

As can be seen by reference to fig. B.6.11, almost every one of these groups occupies a continuous area on

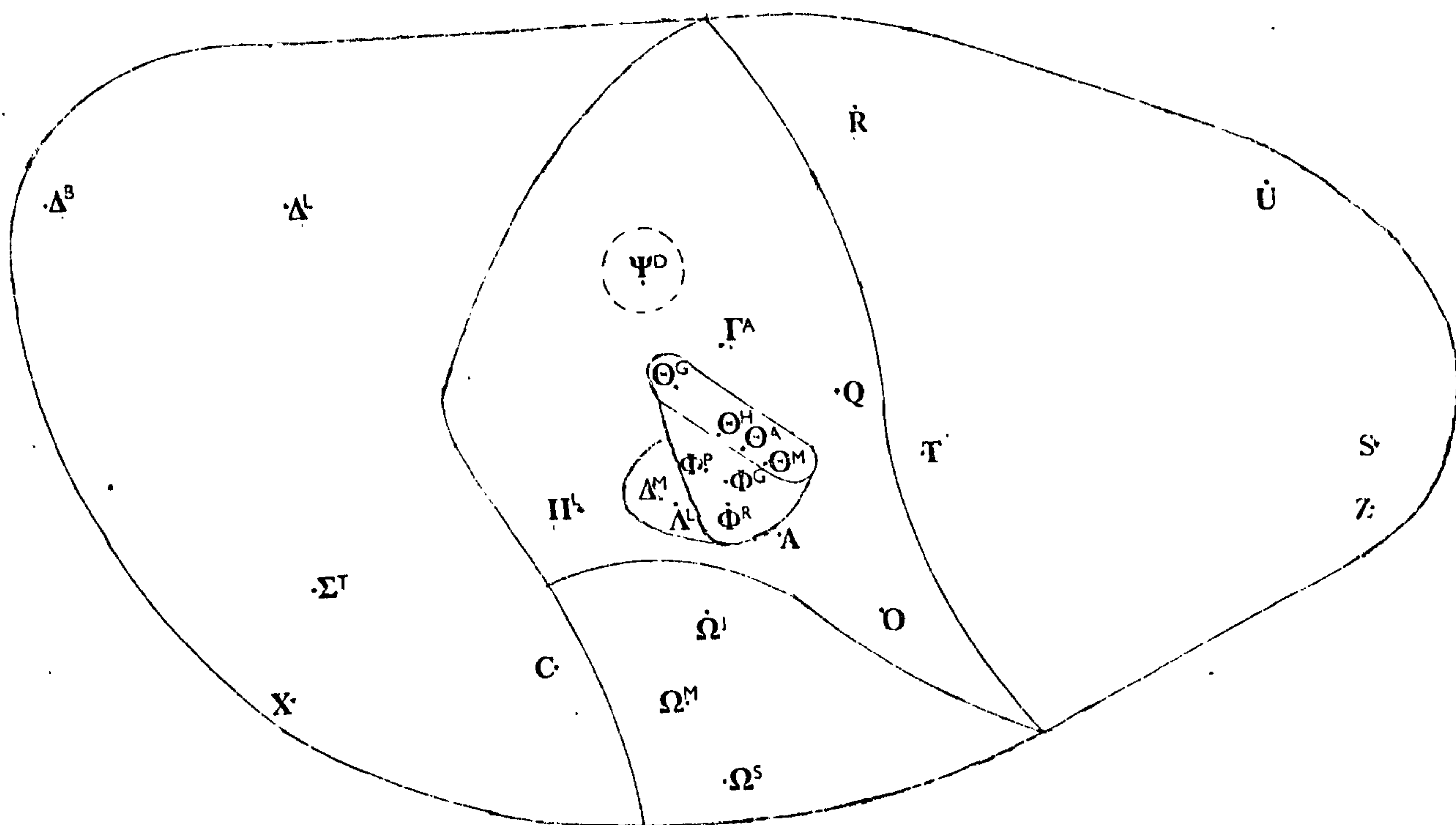


Fig. B.6.11.

the map. The one exception is the Italian group, whose area is broken up by the Visigothic, Theodulfian and Alcuinian groups. Now when we turn to p. liv, we find that the evidence in favour of regarding $AOQ \Gamma^A \Pi^L$ as a group in their own right is not at all solid or clear-cut: the editors were unable to adduce even one passage in which all five agree in error against the rest. It would seem from the map that we should rather define a broad class of Italian texts, varying considerably among themselves. This class will contain not only the five mss $AOQ \Gamma^A \Pi^L$, but also the members of the θ , ϕ and Visigothic groups, plus ψ^D . The groups θ , ϕ and $\Delta^M \wedge^L$ will then be regarded as particular families, falling within the new Italian class¹: but there appears to be little justification for throwing together all members of this Italian class which do not belong to any of these families, as an exclusive Italian group on their own.

One can form a very rough idea of the location of the "best point" (which we shall denote x rather than Ω , the symbol reserved for the Paris text), by considering the latter table on p. xxx, which shows the number of errors in 24 out of our 28 mss. These figures do not refer to

1. With regard to θ and ϕ , at least, one is by no means alone in asserting that their original stock is Italian. Loewe states that Theodulf's "text-type is fundamentally an Italian one" (p. 127). Alcuin's recension is drawn, by and large, from the Northumbrian text, which in turn had been introduced from Italy.

the same sample as our map, and so it seems best to appeal to them in an intuitive fashion. The number of errors contained in the several mss was found to range from 21 to 65. The six mss which offend least are $\phi^G \phi^R(21)$, $A \Delta^M \phi^P(23)$, $\Lambda^L(24)$, all of them members of our extended Italian class. As they lie within a fairly small area of the map, it is tempting to locate x somewhere within that area. However, the margin whereby the figures for the next six mss, namely $\theta^G(27)$, $\theta^M \Omega^J(29)$, $\theta^A \Omega^M(31)$ and $S(32)$, differ from the first six, can hardly be called substantial¹; and some of these mss (cf especially S) are quite distant from the proposed area. Thus it would not be altogether safe, in our present state of knowledge, to assign x to that general area, let alone to a particular spot within it. No doubt a more extensive count of variants would enable us to speak with greater confidence, sufficient for a team to be formulated².

In this brief survey, it has proved possible to do no more than skim the surface of a notoriously complicated subject; but I trust that the potential usefulness of statistical methods in the textual criticism of the Vulgate has been demonstrated. The fact that no such tables appeared in the latest volume of the Rome edition (vol. xiv, 1972: Jeremiah) does not, I hope, mean that this approach has been abandoned altogether.

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1. These figures, it must be remembered, give simply the number of errors; the margin would have been more impressive if they had represented percentages based on a few hundred passages.
 2. The editors appear to treat the triad ACO rather like a "team" (p. lix).

D. GOSPEL OF LUKE

Reference

B.M. Metzger, "The Text of the New Testament", Oxford 1964.

Our last map is based wholly on statistics with which Mr. J.G. Griffith was kind enough to supply me. They refer to the behaviour of fourteen of the best-known mss, over three extracts from Luke, namely 8:5^b-43, 14:1-15:17, and 23:1-40. These figures were utilised, and those for the first extract were published, in Griffith's article, "Numerical Taxonomy and some Primary Manuscripts of the Gospels"¹, to which the reader is referred for details on sources and on policy regarding the admission of variants².

My first attempt to formulate a map came to grief, for the fourteen mss turned out to divide themselves into three classes so sharply that Kruskal's algorithm yielded a trivial map, consisting of three points only, with all the members of any one class located at the same point. I therefore resorted to the device described on p. 4:7. In the resulting map (fig. B.6.12), three distinct clusters

1. J.T.S. (1969), pp. 389-406. On Griffith's own statistical contribution to textual criticism, see Thes., pp. 11:123 ff.

2. In one respect I have departed from that policy. Griffith excluded from consideration all cases in which one ms diverged from all the others, but he made an exception of D; I have ignored all unique readings, including those of D.



D
FIG. B.6.12

can still be recognised, but the points no longer run into one another.

Apart from voicing an overall impression of consistency between this map and the results of investigations on traditional lines, there is little for me to say. D is the only witness, among our fourteen, of the Western text; XBL p⁷⁵ represent the Alexandrian text. The remaining mss are customarily divided into two classes - Caesarean (Θ, fam. 1, fam. 13) and Byzantine (AESWYΠ); in the map, the two classes go to make up the same large cluster, but are not actually intermingled. I did not, of course, venture to locate Ω on the basis of my own judgment; I imagine, however, that the communis opinio would tend to place Ω somewhere in the west (or south-west) of the map¹.

Surveying the results of this whole chapter, we can state that they are in general agreement with the work of earlier scholars, and that at many of the points where they disagree, the conclusion favoured by our method has much to commend it. We can certainly feel sufficiently encouraged to proceed to the tradition which holds the central place in this Thesis: the Peshitta Psalter.

1. In that case, the Caesarean mss will form a band that lies between Ω and the Byzantine mss; may we infer that the Caesarean text is to some extent the parent of the Byzantine?